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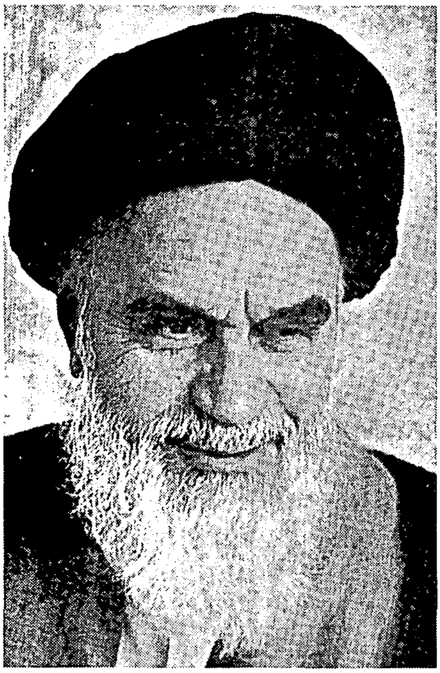

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Coordination versus Prisoners' Dilemma: Implications for International Cooperation and Regimes

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The study of political institutions in general and international cooperation in particular has been beneficially influenced by the Prisoners' Dilemma (PD) game model, but there is a mistaken tendency to treat PD as representing the singular problem of collective action and cooperation. By relaxing the assumptions of 2×2 games and developing an alternate model of the coordination game, I show how some cooperation problems have very different properties from those found in PD. The analytical results of the two games are compared across several important dimensions: number of strategies available, number of iterations of the game, numbers of players, and the distribution of power among them. The discussion is illustrated with specific problems of international cooperation, and the implications of alternative cooperation problems for the formation and performance of international regimes are explored. The basic solutions for PD and coordination have divergent ramifications for the institutionalization, stability, and adaptability of regimes and for the role of hegemony in the international system. However, the coordination model does not replace the PD model but complements and supplements it as a way to understand the diversity of political institutions. These results are widely applicable to areas of politics beyond international relations.

The problem of international cooperation is essentially one of collective action applied to the particular circumstances of the international system. The formation of international regimes raises the same general issues that surround the development of political institutions for resolving collective action problems in various spheres of human activity. The nature of these institutions may range from formal organizations suggested by notions of legal contract and central government through a wide variety of intermediate institutions to the very informal methods of collective organization suggested by norms and conventions (Krasner, 1982a, p. 186). This considerable range of institutional variation has important consequences for regime formation (or expansion) in different international issue areas. It poses the fundamental question of what sorts of regimes are appropriate for resolving particular problems of collective action, while simultaneously it leads to a consideration of the sources of variation among international regimes. The nature and different sources of this variation in regimes will be the focus of this article. Although the discussion has corresponding implications for the study of domestic politics and for political theory more generally, the analysis will be presented in terms of the problematique of "international regimes." The lack of preexisting centralized institutions which characterizes international "anarchy" highlights the differences among collective action problems and among their solutions, whereas the substantive questions of international politics serve to concretize the discussion.

One explanation of institutional variation lies in the possibility that different regime forms may provide different solutions to the same problem; that is, given any particular problem of collective action, a solution might be achieved in any number of different ways and be reflected in correspondingly different institutional development. For example, the problem of maintaining free trade among states might be resolved by formal contract and enforcement through international treaties and organizations. Alternatively, the same trade problem might be resolved through the emergence of norms and conventions that provide informal guides for state conduct. In fact, the current international trade regime seems to fall somewhere between these extremes (Lipson, 1982). Of course, these different solutions are likely to entail other important differences. More formal regimes may have advantages in preventing states from taking noncooperative actions for short-run benefit, whereas less well institutionalized regimes may be better in allowing states flexibility to adapt international norms to domestic needs. Despite

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these variations, the different regime possibilities and combinations are clearly different solutions to the same fundamental problem of maintaining a liberal free trade regime (with its presumably generalized benefits). In this way, the diversity among different regimes could be understood even if the underlying structure of all collective action problems were similar.

A second explanation for regime variation is that regimes in different issue areas represent different solutions to fundamentally different problems. Different histories and contexts of particular international issues may facilitate certain types of solutions in some issues and other solutions in other issues. For example, the historical development of the norm of open access to ocean transportation routes provides a solid foundation for an informal norm-based regime, whereas in international trade, discrimination against foreign goods is a well-recognized right of states, and so the norm of nondiscrimination is inadequate as the sole basis of a regime. But an even more important source of this type of regime variation lies in the possibility that the underlying strategic structure of issues is fundamentally different; this will occur when the nature of interdependence between states, as reflected in the ways in which they affect and are affected by one another, differs across issue areas. If this is the case, then there is no longer any single problem of collective action. Instead, multiple different problems of collective action need to be recognized. These multiple problems may be soluble by similar types of regimes, or they may each require different institutional forms for their resolution.

Both explanations of variation in regime type are important, but the impact of strategic structure is generally less well understood than are other sources of regime variation. In large part this is because a particular model of collective action, the Prisoners' Dilemma (PD), has incorrectly come to be treated as the problem of collective action. Indeed, sometimes even a very special case of PD—the public good problem—is treated as if it were the sole problem.[1] A wide range of issues have been analyzed this way without careful scrutiny to check that their underlying strategic structure corresponds to this particular formalization. Although PD (and public goods) provide an important category of collective action problems, inappropriate analysis and erroneous conclusions result when other issues are treated as if they are PD when in fact they are not.[2] Other collective action problems hold very different implications for the likely nature and performance of regime institutions.

An illustrative example of this problem is found in the application of one of the central theorems of the international regimes literature: the theory of hegemonic stability (Keohane, 1980, 1984; Kindleberger, 1978). Although the theory is ultimately concerned with the dynamics of the world system, its analytical underpinnings are essentially static and based on Olson's (1965) analysis of "privileged groups" in public goods problems.[3] The theory claims that 1) international systems with dominant states are likely to have stable regimes provided by that dominant state; and 2) this will be to the advantage of the smaller states which, in Olson's terms, will exploit the large states by free riding on this provision by the dominant state. The surprising implication is that weaker states benefit from international inequality while hegemonic powers are better characterized as charitable benefactors than imperialistic or exploitative masters. (The dynamic corollary is that the rise or decline of stable international regimes is a response to the rise or decline of a dominant state in a particular international issue.) These are striking claims both in their positive derivation and their normative implications. It is hardly surprising that they have been picked up by a large number of scholars and applied to a diverse range of international issues. Unfortunately, many of these applications have been inappropriate.[4]

The problem with the theory of hegemonic stability does not rest with Olson, because his analysis is correct for the situation he analyzes (i.e., public good provision). Instead, the problem must be parcelled out between the fuzziness and the inappropriateness with which the theory is put forward. First (the fuzziness), it is not clear exactly what is being provided. Much of the discussion makes it seem as though provision of the regime itself (rather than cooperation in the issue area) is the primary concern. If this is the case, then the analysis may well be correct if regime institutions have the properties of a public good. But such an analysis seems to miss the point by concentrating on the relatively minor question of who will bear the costs of supporting the regime institutions rather than on the more important questions concerning the nature of those institutions (and the outcomes thereby determined). Moreover, it divorces the question of regime provision from a consideration of the characteristics of the underlying issue area. This point is directly related to the second point (the inappropriateness) that if the good being provided relates to the underlying issue area, then application of the theory requires that the issue area indeed involve a public goods problem. Many of the issues to which the theory has been applied do not seem to involve public goods. In such cases, it should not be expected to pertain.[5]

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[1] In large part, this may be due to Olson's (1965) persuasive labelling of his analysis of public goods problems as "*The* Logic of Collective Action" (emphasis added). For an analysis of public goods as a subset of PD see Hardin (1971). Below I consider other problems that are equally problems of collective action but involve neither PD nor public goods.

[2] There are important exceptions where different categories of collective action problems have been recognized. See especially Stein (1982), who makes various insightful points about the difference between what he calls "dilemmas of common interest" and "dilemmas of common aversion." His analysis parallels many of the points raised below. For a more analytical treatment see Runge (1981) and Sen (1967).

[3] A group is privileged if at least one member receives sufficient benefit from the public good that he is willing to provide the good himself. Strictly speaking, the privileged group situation is not PD in the range of provision up to that provided by the largest group member. However, the level of provision is still suboptimal, and a PD dilemma persists with regards to reaching the collectively optimal level of provision (Snidal, 1981, chap. 3).

[4] See Snidal (1985) for a more detailed elaboration and analytical critique of hegemonic stability theory.

Therefore, there is good reason to doubt that the theory of hegemonic stability will apply to all international issue areas equally. I consider below a case where the theory does not apply and where different conclusions about the impact of hegemony emerge. This case is discussed as part of a more general investigation of situations of strategic interdependence that result in regime characteristics quite different from those derived under the assumption that the underlying situation corresponds to PD. It provides an understanding of the emergence of different regimes (in part) as a response to fundamentally different problems of international cooperation. As a result, many of the emerging conclusions from the international regimes literature (of which the theory of hegemonic stability is a prominent example) are less widely applicable than is generally believed.

The diverse structures of collective action problems will be analyzed in terms of two simple situations represented in game theoretic format—the PD and coordination games—which pose fundamentally different problems for cooperation. Starting from the basic 2 × 2 representation of these games, the restrictive assumptions of the two-actor, two-choice, one-play game are each relaxed. The implications for (international) political institutions that emerge from these expanded games demonstrates that differences in the underlying strategic structures of international issues provides an important source of regime variation. The final section of the article brings these conclusions together in a discussion of regime institutionalization, regime stability, and the impact of changes in the distribution of power on regime outcomes. Significant differences between the two cases indicate the importance of recognizing that different problems of collective action may require different political resolution if international cooperation is to be successful.

Using Games to Study the Diversity of Collective Action Problems

Ordinal 2 × 2 games provide a useful way to investigate the variety and diversity of collective action problems. Although their use necessarily entails setting aside some contextual and historical richness, the models compensate by clearing away the underbrush of detail to facilitate clarity and rigor in our analysis. Of course they are particularly valuable in directing attention to the strategic aspects of interactions. But several of their major analytical assumptions are overly restrictive for the models to capture important aspects of real world situations. The assumptions of only two actors, each with only two choices and a single interaction between them need to be relaxed to consider a more realistic world of many states, each with multiple choices and repeated interactions.[6] These alternative assumptions will be considered below in looking at the relationship of regime characteristics to the strategic structure of collective action problems.

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[5] No doubt artful reconstruction could make any of these issues seem to be public goods. For example, international cooperation itself can be argued to be a public good since (perhaps by definition) all parties are made better off. But this is only a sleight-of-hand which makes something seem to be a public good by distracting us from the actual properties of the underlying issue over which international cooperation is sought. Note that it could be used to show that all purely private goods market transactions are public goods.

[6] There are a host of other assumptions including, for example, that both players have perfect information concerning the game rules and payoffs, that ordinal payoffs provide an adequate description of the situation, that players are "rational" in the sense of being some sort of consistent maximizing agents, and that all relevant factors in the external game environment have been incorporated into the game. These assumptions may be heroic in certain circumstances and suggest the inadequacy of game models for understanding particular issues in isolation from substantive analysis of those issues. However, game models are useful for understanding that part of regime variation which can be accounted for in terms of differences in the underlying strategic structure across issues.

Considering all possible combinations of ordinal preferences, there are 78 different 2 × 2 games which provide an exhaustive listing of the possible relations of interdependence in a two-state, two-choice, one-play world (Rapoport & Guyer, 1966). To investigate the implications of different types of strategic interdependence for regimes, I consider two of these: the PD and coordination games. Although the choice of these particular games may seem arbitrary (which in itself would not matter for showing that *some* different situations of interdependence lead to different implications for regimes), there are compelling reasons why a comparison of these two games is germane. First, these two games have been widely discussed as important problems both in the game theory literature and in substantive discussions of problems of international cooperation. Second (although it will not be developed here), in a typology of cooperation problems in 2 × 2 games, these two games emerge as the two polar cases of assistance and coordination problems as discussed below (Snidal, 1981, chap. 2). Third, they are analytically more tractable and fruitful than many other simple games.[7]

In this article I concentrate on regimes as ways to facilitate cooperation. As is evident below, this is one important force motivating the formation and subsequent development of regimes. However, there are also regimes where cooperation plays a limited role or even no role (e.g., where one state is able to impose a regime upon another) and the following discussion is not relevant to those cases. Also, the term "cooperation"—and the corresponding references to cooperative choices and outcomes—is used in the common-sense usage of working together for mutual advantage rather than in the misleading game theoretic sense of a cooperative game as one where binding agreements are possible. This latter piece of persuasion definition neatly assumes away most of the tough political problems involved in cooperation, especially with respect to the problematique of international anarchy, where the absence of a centralized capacity to enforce agreements is fundamental.

Finally, the use of game theoretic analysis implicitly entails a series of assumptions about the nature of international politics.[8] Treating states as goal-seeking actors with well-defined preferences implies an essentially realist (or neorealist) view of international politics. Aside from the usual dangers of anthropomorphism, such an analysis is incomplete insofar as either transnational or subnational actors have an independent role in international politics. Indeed, even the assumption that all states have choices to make that matter for one another is nontrivial. It may make the analysis more applicable to North-North or East-West issues than to many North-South issues where the preponderance of effect is unidirectional. Finally, I assume an international political environment where there is communication and sufficient order to make cooperation feasible. However, I do not assume strong central institutions able to enforce agreements strictly (although they are not in all instances completely powerless and, at a minimum, may serve to enhance the self-enforcement of international agreements). With all of these caveats—plus those that necessarily attend the use of simple models to understand a complicated world—I proceed.

The Assistance Problem: Prisoners' Dilemma

PD is well known as a problem of collective action and so is dealt with only briefly here. The normal (or strategic) game form is presented in Figure 1, where policy alternatives are indicated in bold face (**0**,**1**) for each of the two states, and the entries in each cell represent ordinal payoffs for States A and B, respectively. (The ordinal payoffs are represented as ranging from 4 = "Most Preferred Outcome" to 1 = "Least Preferred Outcome".)

PD is an archetypical example of the disjuncture between individual and group rationality which characterizes many problems of collective action: Pursuit of individual self-interest by states (represented by their each choosing their dominant strategy **1**) results in their being worse off than if both abstain from pursuit of their narrow self-interest and cooperate (represented by their each choosing strategy **0**). The dilemma persists even if cooperation is achieved, because both states will continue to have strong incentives to defect and the system is likely to return to the stable noncooperative and deficient equilibrium (i.e., outcome (2.2) in Figure 1).

The PD is the standard representation of externalities (including public goods) where in the pursuit of their own private gains actors impose costs

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[7] The other compelling game which arises for these very same reasons is "Chicken." In terms of the typology of 2 × 2 games it is the case involving simultaneous assistance and coordination problems. It is discussed in Snidal (1981), but is excluded here because it is analytically more complicated than the other two models.

[8] The assumptions described here are not the only possibilities. For a discussion of game theory as a theoretical approach to international politics, see Snidal (in press).

Figure 1. Prisoners' Dilemma

		State B	
		0	1
State A	0	3, 3	1, 4
	1	4, 1	2, 2

on each other *independently of each other's action*; that is, in the pursuit of its national interest State A makes State B worse off regardless of what the latter does, and vice versa. Because each is affected more by the other's move than by its own move (e.g., State A's choice alters B's outcome by two ordinal rankings, whereas B's choice can affect B's outcome by only one ordinal ranking), they both need assistance from the other in the form of forbearance from its individually rational choice. Because each gains more by participating in a program of mutual assistance than by acting independently, there is room for cooperation in PD.[9]

The outcome of the single-shot PD depends heavily on the social-political context. If enforceable agreements are not possible, then neither state will cooperate, because each will be better off not cooperating regardless of what the other does. If binding agreements can be made, then both states will find it in their interests to enter into an agreement to cooperate. This notion of *contracting* out of PD depends on some equivalent to a Hobbesian sovereign able to enforce agreements. The sovereign might accomplish this by forcing states to perform on their promises or by imposing sanctions on failures to perform, so that the strategic structure is changed, and states find it in their self-interest to comply "voluntarily." Obviously such a sovereign is rarely present in international politics. However, as one extends the underlying analytic model beyond the single-shot PD, other factors appear that may allow for less centralized cooperation (e.g., iterated play) or otherwise affect the ease of solution by contract (e.g., bargaining problems in graduated games). Investigating these possibilities requires relaxing the underlying assumptions.

Multiple Choices in the Graduated Prisoners' Dilemma

The assumption that states face only a dichotomous choice is a drastic simplification of the wider menu of choices typically available to states.[10] A simple case in point is that of ocean fisheries, where the choices presented are not simply to fish or not fish any particular species but cover a complete range of intermediate choices expressible in terms of annual catches for each state (and species) involved. In other issues, the available policy choices may be more complex and multidimensional. For example, policy options regarding trade issues include not only tariffs and quotas but an array of techniques ranging from bureaucratic impediments to negotiation of voluntary export restraints and countertrading requirements. However, in relaxing the assumption of dichotomous choice, I assume that these many policy options (or combinations of them) can be arrayed meaningfully along a single dimension. Thus trade policy is treated as ranging from an open-door extreme to a closed-door extreme, with a full array of intermediate policy options in between.

In order to represent this full range of policy options, I use a graduated version of PD as shown in Figure 2. Note that the subgame defined by the two extreme strategies for each side and indicated by the four corner cells is identical to the simple 2 × 2 of Figure 1. Now five policy choices are shown for each state, while further intermediate policy choices could easily be inserted between any two policy options shown. The ordering of these policies (e.g., from "open" to "closed" in trade policy) is reflected in the ordering of the ordinal payoffs shown in the cells of the matrix. For any policy choice by State B, State A makes itself better off and State B worse off by choosing a less-cooperative (higher-numbered) strategy. Again, because the same is true of State B's choice given any choice by State A, there is room for

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[9] This is not true for all cases where one or both states need "assistance" from the other. For example, in the game of "Deadlock" (formed by interchanging the payoff pairs in the northwest and southeast cells of Figure 1), each is better off by foregoing the other's assistance rather than by accepting it at the cost of foregoing its own dominant strategy.

[10] Of course, there may be some cases where choices are strictly dichotomous or where states can deliberately structure the issue to reduce multiple choices to a single binary decision. The latter is especially relevant when threshold effects or focal points are involved. For example, "Mutual Assured Destruction" (MAD) as a weapons strategy is deliberately designed to avoid intermediate-sized nuclear wars by making nuclear war an all-or-nothing event. The examination of these binary choices has been a central theme in the work of Schelling (1960, 1978), which remains the authoritative source on these questions.

Figure 2. Graduated Form of Prisoners' Dilemma

				State A		
		.0	.25	.5	.75	1.0
		K		L		
	.0	3.0, 3.0	2.5, 3.3	2.0, 3.5	1.5, 3.8	1.0, 4.0
	.25	3.3, 2.5	2.8, 2.8	2.3, 3.0	1.8, 3.3	1.3, 3.5
State B	.5 J	3.5, 2.0	3.0, 2.3	2.5, 2.5	2.0, 2.8	1.5, 3.0
	.75	3.8, 1.5	3.3, 1.8	2.8, 2.0	2.3, 2.3	1.8, 2.5
	1.0	4.0, 1.0	3.5, 1.3	3.0, 1.5	2.5, 1.8	2.0, 2.0

Numerical entries should be treated as indicating only ordinal preferences where, for example, 3.5 is preferred to 3.25.

cooperation throughout the policy ranges available to the two states. This is a significant feature of graduated PD that every subgame (as defined by considering any two policy choices for each state) is also a PD. Thus the strategic structure of an issue is not altered by looking at gradations of strategy or, equivalently, *PD is the same in the small as in the large.*[11]

Although the basic strategic structure of the game remains the same when intermediate policy options are introduced, the game is altered in two potentially important ways. First, a bargaining problem arises which is not present in the 2 × 2 version of PD. In the simple game there is a single cooperative and efficient outcome at the top left hand corner (3.3). But in the graduated game there is a larger set of available cooperative (and Pareto-efficient) outcomes as represented by the dark line segment $\overline{JKL}$ in Figure 2. Although agreement at (3.3) may seem compelling the way the game is set up, either state may attempt to bargain for a better outcome along $\overline{JKL}$, introducing new complications to impede cooperation.[12] A second difference between cooperation in the simple and graduated games occurs when graduated policies allow states to achieve intermediate levels of cooperation in circumstances in which they are unwilling to risk complete cooperation. One or both states might be unwilling to risk full cooperation (strategy 1) because they cannot accept any possibility of receiving their worst outcome (i.e., a payoff of 1) in the event that the other state does not cooperate. In graduated PD, states may be able to achieve intermediate levels of cooperation (e.g., each play strategy .5) where the outcome if the other does not cooperate is not as disastrous (i.e., an outcome of 1.5).[13] Such intermediate possibilities for cooperation also open up opportunities to learn cooperative behavior in a dynamic game setting. However, despite these differences, the important observation remains that the graduated PD preserves the basic structure of the simple PD game.

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[11] Technically, this result follows because PD falls into the category of separable games where the impact of each player on the other can be assessed independently of what the other does. See Hamburger (1969) and Snidal (1981, chap. 3).

[12] The (3.3) outcome only looks compelling because the game is perfectly symmetric and information about payoffs and strategies is perfect. In real world situations, the lack of such a clear delineation of "most cooperative strategy" and differences in intensities of preferences are likely to obscure any "obvious" single solution. Technically, the difference rests with the fact that when the "core" contains only one point (as in Figure 1), it provides a compelling single solution, but when it has multiple cooperative points (as in Figure 2), it is indeterminate as to the precise outcome.

[13] This difference will not be significant in single-shot PD where any cooperation is unlikely—but it may be significant in iterated PD discussed below. In "Chicken" these graduated strategies are crucially important and change the nature of the game even in the single-shot game (Snidal, 1981, chap. 3).

Multiple States and Asymmetries in Prisoners' Dilemma

The extension of the two-actor PD to the *n*-actor situation is straightforward and well known; it corresponds to the standard case of (separable) externalities where each state imposes costs or benefits on others independent of their own behavior. In terms of ordinal preferences, every state is in the same symmetric relation to every other state (although this symmetry may break down under interval-level preferences where intensities are taken into account). This extension also poses no problem in the graduated game where it can be depicted in terms of an *n*-dimensional "cube" where each dimension represents one of the *n* state's policy alternatives and the outcome is represented by the point in the cube (and corresponding payoffs) determined by the choices of all *n* states. Standard examples include the "tragedy of the commons" where every state has an individual incentive to overexploit the commons even though this is contrary to the collective interests of all states.

The two standard results from *n*-actor PD are: 1) that larger numbers of states inhibit cooperation; and 2) that asymmetries (usually implicitly defined in terms of the impact of one state on another but, in fact, also a result of differences in preferences and intensities) facilitate cooperative and efficient outcomes. This latter result is often elaborated with the specific claim that smaller countries will benefit disproportionately in such asymmetric situations (i.e., the small exploit the large). However, these results are based specifically on public goods assumptions and are not valid as universal generalizations for all PD. Again, confusion of public goods as the problem of collective action is misleading for understanding the diversity of cooperation problems. But a precise specification of the conditions under which these generalizations apply does provide further insight into international cooperation. Not surprisingly, these conditions are closely related to the defining characteristics of public goods of "jointness" (i.e., that consumption by one state does not diminish the quantity or quality of the benefit available to the other) and, especially, of nonexclusion (i.e., that there is no way to prevent any state from receiving benefits).[14] When these conditions do not apply—and PD problems need involve neither jointness nor nonexclusion—then the generalizations need to be qualified or changed.

First, consider the claim that larger group size impedes cooperation. Insofar as this refers to the problems of organization that arise as a result of the different social-political features of large as opposed to small groups, the assertion is valid. As the number of members in any social grouping increases, there is likely to be a commensurate increase in problems of communication, in the tendency for states to perceive that their individual actions are less visible so that they can renege or cheat on cooperative arrangements without being noticed, and in the problems of resolving bargaining disputes over the distribution of benefits from cooperation. Even with an extensive and institutionalized apparatus for negotiation and diplomacy, states are not able to overcome such problems easily. Therefore, in general, multilateral negotiations involving greater numbers of states can be expected to increase the difficulty of cooperation.

However, an increased number of states also may have an opposite effect by opening up new areas (or levels) of potential cooperation which are not accessible to smaller numbers of states. There will be some projects on which no small group of states has incentive to cooperate but on which a larger group will find room for cooperation (e.g., projects such as the European airbus might not have been undertaken if, for example, there had been only two interested states). Even in public goods problems, an increased number of participants will not only increase the problems of organizing collective action, it will also increase the benefits from cooperation as reflected in the corresponding increase in the optimal level of provision. This beneficial impact of increased numbers of states may hold independently of any assumptions of jointness (e.g., where there are economies of scale in cooperation), but jointness will further enhance incentives for cooperation.

The possibility of exclusion will be especially important in mitigating the adverse effects of increased numbers of states on the prospects for international cooperation. If the benefits of cooperation can be restricted to states that behave cooperatively, then such exclusion will make provision in large groups hardly more difficult than in small groups. Indeed, in some sense exclusion makes the problem a small group one of agreement between any single country and the group of already-cooperating states. Admission to the group of benefitting states (or continued membership in the group) will be made contingent upon contribution to the collective effort. Many international organizations involving such diverse issues as trade, money, military alliances, or health regulation are in fact able to practice such exclusion to different degrees. Although difficult bargaining problems over what constitutes a "fair" contribution by various members persist, such exclusionary principles do facilitate substantial levels of cooperation.

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[14] I use the term "closely related" here because there are some subtle distinctions between the technical and institutional aspects of the relevant restrictions. Within the analysis of market economies, nonexclusion is treated as a technical property of goods (within an otherwise perfect set of property rights), whereas at the international level, problems of nonexclusion may also arise as a consequence of political-institutional factors (e.g., lack of control over exclusion reflected in imperfect property rights). Furthermore, the concepts of jointness and nonexclusion are not analytically distinct since jointness is a necessary but not sufficient condition for nonexclusion (Snidal, 1979). Such distinctions are essential for a more fine-tuned analysis of particular issue areas, although for the more general analysis below we will concentrate on the property of nonexclusion.

Second, consider the impact of asymmetry within a group of states.[15] Hardin (1982, pp. 40-41, 73) demonstrates that the likelihood of cooperation depends not only on the size of the group but on the net benefits received by each cooperating member and hence on the minimum-sized subgroup that can benefit from cooperation. For a given average payoff, a more asymmetric distribution of benefits means that a smaller subgroup is able to benefit from cooperation and therefore that cooperation within the group is more likely. If the issue involves public goods, then other states can get a free ride off this provision (and the distributive implications of the theory of hegemonic stability pertain); if the good is not joint, then there will not be benefits to noncontributors and only the larger states will benefit from their ability to cooperate; and if the good exhibits jointness but exclusion is possible, then noncontributors may be induced to contribute to the cooperative venture (perhaps at a cost they may see as exploitative).[16]

Therefore, the general conclusions that cooperation in PD is less likely as group size increases or as asymmetry decreases depend heavily on the exact context of the issue. These conclusions are not universally applicable whenever the issues do not meet the public good conditions of jointness or nonexclusion or both. Moreover, sometimes the cooperation that large numbers serve to impede is only a possibility because there are a large number of states involved in the issue. Only with these important caveats, can one conclude that *in PD large numbers tend to inhibit cooperation.* In addition, *asymmetries increase the likelihood of some cooperation but the distribution of benefits from cooperation will also be affected by the asymmetry.* Sometimes the distribution will favor smaller states, but in other cases they will favor larger states. Thus generalizations such as the "theory of hegemonic stability" need to be tightly circumscribed by careful specification of the properties of particular issue areas. This is true even within the realm of PD situations, and I demonstrate below that it applies even more forcefully in the coordination problem.

The Role of Time in Prisoners' Dilemma

One limitation of 2 × 2 games is that their inherently static, one-shot nature is inadequate for analyzing problems where considerations of play through time are important. Although it falls short of a truly dynamic analysis, some understanding of cooperation in a more dynamic world is provided by considering the situation where the same 2 × 2 game is played (or "iterated") through time.[17] Normally this involves looking only at the impact of future time, but it is also relevant to look at the impact of past and present time as discussed below.

It is now widely understood that the prospect of repeated play into the future increases the likelihood of cooperation in PD even in the absence of centralized enforcement (Axelrod, 1984; Hardin, 1982; Shubik, 1970; Taylor, 1976). Although noncooperation is a rational strategy in single-play PD, repeated play of the game into the future makes it worthwhile to take a chance on cooperating today in the hope that other states will do likewise and mutual cooperation will prevail over the deficient noncooperative equilibrium in the longer term. This possibility is more likely: 1) the longer the time horizon (or expected number of iterations of the game), since, as the final game approaches, the incentive to cooperate because of future considerations diminishes; 2) the lower the temporal discount rate or, equivalently, the more future benefits from cooperation are valued relative to the prevailing incentive not to cooperate; and 3) the greater the benefits from cooperation relative to the gains to be had from not cooperating (or the losses to be incurred from cooperating while other states do not).[18] To the extent that these conditions are fulfilled, *the fact that a PD issue is recurring through time will facilitate cooperative behavior.*

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[15] In doing this I implicitly move from ordinal to interval-level payoffs.

[16] Smaller states will be willing to join at any cost less than their benefit from participating in the cooperating group. As a result, imposing sufficiently high admission costs on smaller states will have the effect of ensuring that all of the profits of cooperation (i.e., excess of benefits over costs) accrue to the larger states.

[17] Iterated games have important limitations as approximations of dynamic situations since, for example, they do not allow for the possibility that the structure of the system itself may change over time. This is an important consideration when we are trying to analyze issues where institutional and political changes may be involved. Ultimately the supergame formed by looking at simple games iterated through time is only slightly more dynamic than the simple game itself (where strategies can be thought of as dynamic insofar as they represent a listing of how the state will act through time). Nevertheless, the supergame does provide an important first step in looking at the impact of playing through time.

If we treat "present" time as the duration of a particular simple PD game embedded within an ongoing issue area, it becomes apparent that the speed with which states can adjust their policies (i.e., the duration of any single PD) has an impact similar to that of a longer future time horizon. The faster that states adapt to each others' actions, the more iterations of the game there are in any fixed length of time. Thus *when states in PD can adapt quickly to cooperative or noncooperative actions by the other side, there are greater prospects for cooperation* for precisely the same reasons that future play improves prospects for cooperation in PD. Improved information and communication—including preparations for policy changes—may be an important aspect of regimes for this reason.

Finally, past experience in PD may either encourage or hinder cooperation because of the mixed effects of learning and building trust between states. This is directly related to the incentives to cooperate because of "reputational" effects that encourage cooperation when games are repeated into the future. Generally, past success at cooperation will positively reinforce cooperation because of the increased reputational costs of reneging on longstanding arrangements. However, past success at cooperation also enhances the stability of a regime so that states may begin to feel they can cheat on cooperation without disrupting the tendency of other states to cooperate. This will be most likely in large number situations involving public goods and is well exemplified by Russett's (1970, p. 99) observation that the tendency of small allies in NATO to try to free ride on U.S. military provision provides strong evidence of the past success of the alliance.

In summary, the overwhelmingly most important aspect of time in PD is the expected duration of an issue into the future. Present time—as reflected in the speed of adjustment in responding to other states' policies—plays a similar role in facilitating cooperative behavior. Past time plays a relatively small role.

The Coordination Problem and the Coordination Game

The coordination problem is generally less well understood as a problem of collective action and so will be dealt with in some detail here. Sometimes coordination is presented simply as the problem of two or more actors matching policies where they are indifferent about where they match (or have perfectly correlated preferences over alternative matches). A good example is a meeting problem where two friends are indifferent about where they meet for lunch as long as they both go to the same restaurant. Here there is no disjuncture between individual and collective rationality and no problem of collective action. It requires no more than communication and common sense to achieve an outcome that is both individually and collectively optimal.[19]

A more deep-seated problem of coordination arises when actors have a strong desire to coordinate but some differences over exactly where to coordinate. This problem is introduced into the meeting problem when friends wish to meet for lunch but each prefers meeting at a different restaurant or at a different time. The normal form of this game is presented in Figure 3.[20]

This game poses a different problem of collective action than does PD. The problem in PD is that in pursuing its own self-interest, each state imposes costs on the other independent of the other state's policy choice, whereas *in the coordination game each imposes costs or benefits on*

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[18] The formal conditions for cooperation induced through repeated play are quite stringent and would not appear to offer much help for international cooperation (Taylor, 1976). In large part this is because the purely logical analysis misses some of the important features of the social context which facilitate cooperation, and perhaps even distorts the nature of the problem (cf., Hardin, 1982, chap. 9). Other extensions of the basic supergame model may also enhance possibilities for cooperation (Axelrod, 1984).

[19] Two important discussions of the coordination game are Lewis (1969) and Schelling (1960). Their examples of such coordination problems all revolve around problems of information and communication. Although these are certainly important in some international problems (e.g., see Schelling's military examples), they are not widely applicable to issues of longer-term cooperation failures in regimes where communication is possible.

[20] Figure 3 shows one symmetric version of the coordination problem. Minor variations (and different coordination games) can be created by interchanging the two lowest payoffs in various combinations, although not all of the resulting games are symmetric. However, the basic analytical results are the same as reported here.

Figure 3. Coordination Game

		State B	
		0	1
State A	0	4, 3	2, 2
	1	1, 1	3, 4

the other contingent upon the other's policy. The collective action problem is that neither state can choose its best policy without knowing what the other intends to do, but there is no obvious point at which to coordinate. Easy resolution of the problem (as in the simple meeting problem) is hindered by the inherently opposed interests about where coordination should occur. (State A prefers the northwest cell and State B the southeast cell.) However, once a cooperative solution is achieved, it does not need to be enforced (as did the cooperative outcome in simple PD), but is self-enforcing, because neither state has any incentive to depart from the coordination point in the single-play coordination game. Thus one can appreciate that the problem of coordination is fundamentally different from that of assistance in PD. (Stated more formally, the problem in the coordination game is one of choice between multiple stable and efficient equilibria over which states have opposed interests, whereas in PD the problem is getting away from a single stable but inefficient equilibrium.)

Cooperation in the coordination game depends on the surrounding social-political context. Established traditions or shared principles are likely to provide the basis for the emergence of norms or *conventions* which will guide individual national behavior in an issue area. No centralized enforcement is necessary, because neither state has incentive to depart from an established convention: The prevailing expectations of how the other state will behave which are conveyed by the convention provide a compelling reason for each state to adhere to it in order to gain the benefits of coordination. Any role for centralized authority in coordination problems is likely to be less concerned with enforcement than with codification and elaboration of an existing or latent convention and with providing information and communication to facilitate the smooth operation of the convention. Moreover, because the need for enforcement that exists in PD is absent, in coordination states will be more resistant to surrendering autonomy to a central agency because an alternative decentralized resolution is available. Finally, unlike PD where an obvious "cooperation point" exists, a sovereign would have no clear criterion (independent of conventions) upon which to base a choice between alternative coordination points. Therefore the simple coordination problem is almost certain to result in a decentralized solution.

Although this simple model of coordination applies to an important class of international cooperation problems (e.g., standardization of measurement systems, standardization of equipment within NATO, or recent European attempts to adopt common standards in telecommunications equipment), it is far too restrictive and unrealistic for many other important problems of international coordination. Consider the nature of attempts at international economic cooperation reflected in the recent string of economic summit meetings between leaders of the largest capitalist economies. In essence, the shared objective has been to agree on broad macroeconomic objectives and to coordinate policies in order to achieve them.[21] Two important differences distinguish this situation from the simple coordination game presented in Figure 3: 1) available policies are neither dichotomous nor discrete but involve states choosing over a broad policy continuum (e.g., from "contractionary" to "expansionary"); and 2) coordination is not an all-or-nothing problem and there is no need to meet exactly, provided that policies fall within a range of compatibility. Instead, states might be better characterized as preferring to coordinate policies as closely as possible where, in general, closer is better, *ceteris paribus.* However, at the same time different states will prefer to coordinate at different points (e.g., the West Germans may have a relatively greater concern with price stability than the French) and so some opposition of interests remains within the overall desire to coordinate.

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[21] Of course, many other international economic issues—for example, beggar-thy-neighbor trade policies—will fall into the PD category of collective action problem. But problems such as demand management are closer to the coordination problem. For a discussion of this see Whitman (1977) and Gordon and Pelkmans (1979, part II). Pelkmans's argument is particularly relevant in distinguishing the different nature of international economic problems of what he calls "foreign economic policy" such as trade policy and (international) "demand management policies." He argues that the former require mutual assistance (or "negative cooperation") in the form of mutual restraint from policies that hurt others; the latter require coordination (or "positive cooperation") and meshing of policies between states. However, he fails to draw the corresponding analytical distinctions and often seems to be treating both problems as equivalent to PD.

This conception of the coordination problem provides interesting conclusions for the nature of regimes as I turn to extensions of the simple coordination game.

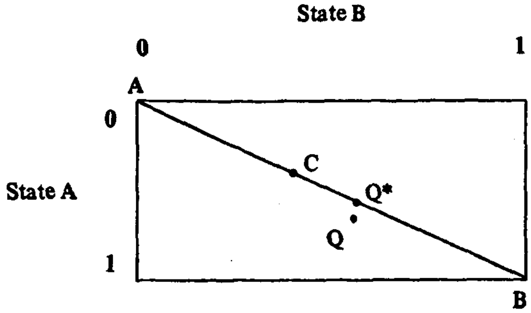
Multiple Choices in the Coordination Game

A modified version of the standard 2 × 2 coordination game is presented in Figure 4. The locus $\overline{ACB}$ is the axis of perfect coordination of policies between two states. However, there are differences over where to coordinate so that State A prefers points on the axis which are closer to A where the payoff is (4,3), whereas B prefers coordination outcomes closer to B where the payoff is (3,4). An intermediate point such as C where the payoff is (3.5, 3.5) might provide a compromise coordination point which would ease resolution of the coordination problem in the absence of a convention (but might complicate it if a different convention already existed). Nevertheless, a point like Q, which is not a point of perfect policy coordination, might be preferred to C by State B because it is sufficiently closer to B's most preferred outcome at B to compensate (from B's perspective) for the deviation from the coordination point. Thus bargaining over the cooperative outcome need not be restricted simply to points on the coordination axis, is as demonstrated below. (However, I assume that for every point Q off the coordination axis there is at least one point on the coordination axis, Q*, which is preferred by both states; that is, the coordination axis is the core of the coordination game.)

Because the graduated form of coordination games is more complicated than that of PD games, it is instructive to derive it using national indifference curves based on assumptions about the nature of coordination problems.[22] Because a state's utility depends on both the distance of the outcome from its most preferred outcome and the distance of the outcome from the coordination axis, its indifference map is reasonably represented by a set of elliptically shaped indifference curves. These curves are concentric to the state's most preferred point and stretch along the coordination axis as shown in Figure 5.[23] Ellipses closer to the center represent higher utility levels, while the more concerned a state is with coordination (relative to its concern for proximity to its optimal point), the more elongated and less circular will be its indifference ellipses.

An analysis of these indifference curves shows that despite the importance of coordination in determining a state's welfare, some incentive to depart from any coordinated outcome may persist. In Figure 5, the broken line $\overline{RR}$ is a reaction curve showing A's best policy given any choice by

Figure 4. Coordination Axis in the Coordination Game



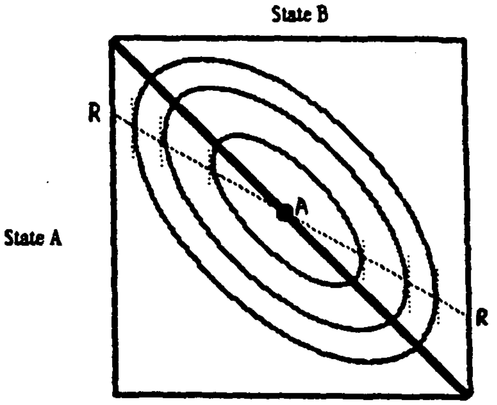


Figure 5. Indifference Curves and Reaction Curve for State A

Point A is State A's most preferred outcome and the center of the indifference ellipses that stretch along the coordination axis (i.e., $\overline{ACB}$ in Figure 4) which is their principal axis. State A's reaction curve (broken line $\overline{RR}$) shows A's optimal choice *given* any choice by B. It is found by locating the point of tangency of any policy of B (represented by the dotted vertical lines) with the indifference ellipse closest to A. This provides the locus of A's optimal response to any policy choice by B.

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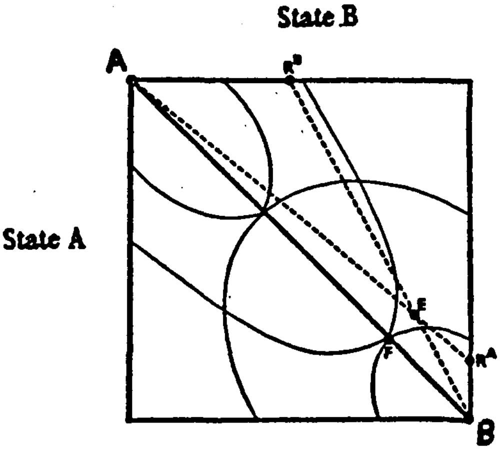
[22] Technically, the greater complication arises directly from the nonseparability of coordination games. The graduated version developed below is not unique but is the substantively most appropriate and fruitful (Snidal, 1981, chap. 3).

[23] If utility is a linear function of the two distances squared, then the indifference curves will be proper ellipses. Otherwise, roughly elliptical curves provide a useful representation for this sort of coordination problem.

State B. Except when B has chosen that policy which corresponds with A's optimal outcome (i.e., at the center of the ellipses) State A has an incentive to depart slightly from the coordination locus in order to get closer to that optimal point (although balancing this against the cost of increased departure from the coordination axis), and to increase its overall utility. State B will have a similar tendency to depart from the coordination axis, and its reaction curve can be derived in a like manner.

Figure 6 presents the joint indifference map for the two states and their corresponding reaction curves. Each state has a most preferred outcome at one end of the coordination axis and elliptically shaped indifference curves along the axis. (There is no need to show the coordination axis beyond these two points because neither state would prefer coordination points outside this range to ones available between their respectively optimal points.) The sets of indifference curves are tangent to one another along the coordination axis which represents the set of core (or Pareto efficient) outcomes. The two broken lines are the reaction curves which show how states would adjust to each other. Their intersection at E represents a (Cournot and Nash) equilibrium outcome. It is striking that this equilibrium point is not on the coordination axis. Instead there is a stable equilibrium which is deficient since there are other outcomes (e.g., at F) that would be preferred to it by both states.[24] Inspection of the indifference maps shows that E is in a PD relationship to F.

Figure 6. Joint Indifference Map and Reaction Curves for States A and B



Lines $\overline{AR}^A$ and $\overline{BR}^B$ are reaction curves for States A and B respectively. Their intersection at E is a stable (Cournot) equilibrium. The deficiency of *E* can be seen by observing that a point such as *F* lies on a higher indifference curve for both states. To see that *E* is in a PD relation to *F*, construct the subgame defined by the rectangle of which *E* and *F* are opposite corners.

This provides a significant insight to the nature of coordination games: *Coordination is a fundamentally different problem when viewed in the small than when viewed in the large.* Indeed, coordination is an inherently large-scale problem which "disappears" when examined in terms of the smaller-scale policy adjustments observed in the graduated game. But the fact that the problem in the small within the coordination game is actually PD does not mean that it can be dismissed as a separate problem or subsumed under PD. Instead, coordination has fundamentally different implications for the formation of regimes which now have to manage simultaneously the need for coordination in the large and the incentives to depart from perfect coordination in the small. Thus the meeting problem of the 2 × 2 coordination game which easily could be resolved by communication and reliance on conventions becomes much more deepseated in the graduated game. Further differences and complications emerge as one investigates other extensions of coordination games.

Multiple States and Asymmetry in Coordination Games

The extension of the coordination game to the *n*-actor case is not as straightforward as the corresponding extension for PD. The simple 2 × 2 game can be expanded to a symmetric *n*-dimensional cube where there are *n* coordination points, each of which represents the outcome preferred by one state. (A fanciful example might be the choice of a single global language.) Although such coordination problems are conceivable, they do not correspond to many substantively important situations as discussed above. A simpler and more useful representation of the multi-actor coordination problem can be developed from the coordination axis in Figure 4. Here states see coordination not as a matter of agreeing between separate and unrelated coordination points but as a problem of meeting on a coordination axis (located in the

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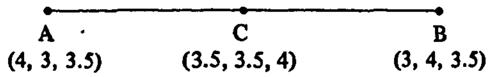
[24] The Cournot behavioral assumption (that each state assumes the other will not respond to changes in its policy choice) has severe limitations in a game situation when we are considering strategic actors. However, the basic nature of the graduated coordination is independent of this assumption, which serves only to illustrate the sorts of deficient outcomes that can occur. Other assumptions incorporating more sophisticated notions of dynamic bargaining behavior (e.g., that both sides behave as Stackleberg leaders) generate even more preverse and unstable outcomes.

Figure 7. Asymmetry of Three-State Coordination



n-space spanned by their individual policy-choice dimensions) with each having its most preferred outcome somewhere along that axis. Again, each state's utility depends on the closeness of the outcome to its optimal point and on the closeness of the outcome to the coordination axis.[25]

A first difference from PD arises with respect to asymmetries among states. Whereas the *n*-state extension of PD could in principle be fully symmetric, the extension of coordination is necessarily asymmetric. Consider the coordination axis $\overline{ACB}$ of Figure 4 when I add a third state whose preferred outcome lies between *A* and *B,* say at *C,* and whose payoff is shown as the third element in the payoff set. The coordination axis now looks like Figure 7. The new state will be advantaged relative to the other states (*ceteris paribus*) since it will necessarily receive an outcome with an ordinal ranking of at least 3.5 (and probably higher), whereas at most one of the other countries will do as well. States that happen to have most preferred outcomes closer to the middle of the coordination axis are in an inherently better position than states with more extreme preferences. Thus coordination problems are almost certain to put states in nonsymmetric relations with one another.

A second source of asymmetry arises when some states have more impact on the outcome than do others. In PD this typically arises because some states impose relatively greater costs (or benefits) on others through their choice of policies. In coordination, the issue is not one of imposing externalities but rather one of the relative need to coordinate. The policy choices of some states will be inherently more significant than those of others and thus provide a relatively greater need for coordination. In general, the need for coordination between any two states will vary directly with their levels of interdependence upon one another. For example, the typical advanced industrial country will find coordination with the largest industrial powers (United States, West Germany, Japan) more important than coordination with the smaller economies (Denmark, Canada, Netherlands).[26] This is likely to give larger countries an advantage in negotiations over international coordination.

The advantage of larger states in international coordination problems is further reinforced to the extent that they are less interdependent with other states and hence less reliant on coordination. This point is illustrated in Figure 6, where State *B* is substantially less concerned with coordination than is State A, as is reflected in its more circular and less elongated indifference ellipses. As a result, the noncooperative (Cournot) outcome of this situation at *E* is substantially closer to State *B*'s most preferred outcome than to State *A*'s. It is reasonable to suppose that this would constrain bargaining over cooperation so that any fully cooperative outcome would fall at a point on the coordination axis which also favored State *B* (e.g., point *F*). Thus, unlike PD where asymmetries sometimes favor smaller and less influential states, *in coordination asymmetries favor larger and more influential states.*

The impact of increasing the number of states involved in a coordination problem is also somewhat different from the case of PD. Again, some of the social-political features of large versus small groups which make organization difficult in PD have a similar effect in coordination. To the extent that large groups impede communication and complicate bargaining, this will make agreement on a coordinating convention more difficult unless tradition or particular aspects of the situation offer an "obvious" (focal point) solution. However, the consequences of reduced visibility of the individual actions of states in larger groups will not be as serious in coordination as in PD. Although it will give individual states an opportunity to depart slightly from the convention (i.e., in the small as represented by the PD zone in Figure 6), the nature of the coordination problem is such that this will not be a problem in the large since national incentives are to coordinate voluntarily with other states. Thus larger numbers of states will give rise to communication and other problems of organization in coordination but not to the strategic incentives to act uncooperatively which are pervasive in frustrating cooperation in *n*-state PD.

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[25] This is a drastic simplification. The coordination axis might not be a single line but a plane or hyper-plane in *n*-space. The assumption implies that all states perceive the issue as involving a single dimension of coordination and that their indifference curves are ellipsoids with the coordination axis as principal axis.

[26] As always, such generalizations need to be treated with caution and appropriate attention to particular contexts. For example, the United States may have a relatively greater need to coordinate with Canada than with larger European states because the two North American economies are so closely intertwined. This is also a good example where the need to coordinate is not symmetric, because the Canadian need to coordinate with the United States greatly exceeds any reciprocal American need to coordinate with Canada.

In terms of the ellipsoid-shaped indifference curves, greater need for coordination with a country is reflected in a shorter minor axis in the plane determined by the two countries' policy dimensions.

Indeed, an increased number of states will reduce the likelihood of the breakdown of cooperation resulting from the deliberate actions of states. Once a coordination point is established, there may be a strategic incentive for each state to depart (or threaten to depart) from the coordination point intentionally in order to force others to accept its preferred coordination point or none at all. However, as the number of states involved in a coordination problem increases, then the impact of any single state and hence the likelihood that it could blackmail the others in this way decrease dramatically. Thus, *in coordination, larger numbers of states do not impede cooperation to nearly the extent they do in PD and facilitate cooperation in some cases.*

Two final points are relevant to the question of increasing numbers of states and asymmetry in coordination. First, exclusion does not play an important role (as it did in PD) for the simple reason that it is not a relevant option in coordination. In PD, costs and benefits are imposed independently of the other's actions so that exclusion (if possible) can affect one state without affecting another. In coordination, costs and benefits are imposed contingent upon the other's actions so that the other cannot be excluded without in some sense excluding all. Thus the very essence of the coordination problem is that exclusion (or free riding) is neither feasible nor desirable. Second, whereas in PD asymmetry among states facilitates cooperation by lessening communication and bargaining problems among the most relevant set of actors, in coordination it decreases cooperation by increasing the strategic incentive to bargain over the coordination point. The exception is when a single state is sufficiently dominant that it can unilaterally determine the coordinated outcome. Moreover, unlike some cases of PD, large countries will tend to benefit more from these agreements since they will have a greater voice in determining the point of coordination. Of course, small countries will be better off than if no coordination was achieved.

The Role of Time in Coordination Games

Because of the inherently stable nature of single-play coordination, playing through time has much less impact than it does in PD. But to the extent that they are important, the three different aspects of time—past, present and future—have substantially different impacts on coordination games than on PD. The greatest similarity of impact is with respect to the length of time that the game has been played in the past. Because cooperation in coordination depends heavily on the ability to agree on common conventions or norms, *past experience* with and learning of norms and conventions *will be very stabilizing for regimes.* The incentive for states to cheat slightly on the coordinating convention in the small may be enhanced by this greater stability of the regime. However, states would have to weigh these smaller gains against the impact cheating might have in diminishing the clarity of well-established norms and hence of upsetting the much greater benefits of coordination in the large.

The role of future time will be the exact opposite of that in PD. In PD the prospect of repeated play provides states with an incentive to cooperate that they do not have in a one-shot situation. In coordination, the single-shot outcome is likely already to be a cooperative one because the ability to coordinate somewhere is more important than are relatively minor differences about where to coordinate. A longer duration of the game gives states incentives to be more concerned with the exact distributional consequences of particular coordination outcomes. In the extreme, it may induce them deliberately to upset prevailing coordinated outcomes in an attempt to institute a movement to other conventions which are more favorable to them. The prospects for such strategic action clearly rest on the (discounted) relative value of future versus present outcomes and the perceived prospects of implementing a change. Thus *playing through time may be destabilizing in coordination problems,* but these considerations will still typically be dominated by the overall stability of the coordination situation.

The impact of present time follows directly from a consideration of the incentives to upset a prevailing coordination outcome for strategic reasons. If adjustment of policies is a lengthy process, then attempts to initiate change in coordinating conventions will necessarily entail greater costs from a longer period of noncoordination during the adjustment process. In general, this should discourage states from trying to change the convention and thereby enhance the stability of the regime.

Two Different Regimes: Coordination and Assistance

Table 1 summarizes the main differences between the problems of collective action posed by coordination and PD. The entries are necessarily simplified and do not capture all of the nuances that may occur within each basic game situation according to particular contextual differences. As

Table 1. Summary of Key Differences and Similarities between PD and Coordination

	Prisoners' Dilemma	Coordination
Basic problem	Assistance: states interdependent because each imposes costs on the other independent of the other's actions	Coordination: states interdependent because impact of each state's choice contingent on other's choice
Basic solution	Contract	Convention
Effect of graduation	Strategic structure unaltered: game is same in small as in large	Strategic structure altered: game is fundamentally different in small than in large
Effect of multiple players	Generally inhibits cooperation: alleviated by asymmetries and possibility of exclusion	May inhibit formation of regimes; enhances stability of regimes
Effect of asymmetries	i. Facilitates cooperation ii. Without exclusion favors smaller states; with exclusion favors larger states	i. Slightly facilitates cooperation ii. Favors larger states
Impact of time	Future time and rapid adjustment increase prospects for cooperation	Past time increases whereas future time and rapid adjustment decrease prospects for cooperation

a result, the differences between the two situations are drawn somewhat more starkly than they may actually appear in real world situations. Nevertheless, the table usefully summarizes the fundamentally different natures of the two problems of collective action.

In comparing these two problems of collective action, we have also (directly and indirectly) been comparing the problems each poses for international cooperation and the implications for resolution of these problems through international regimes. For example, the discussion of the impact of graduated policy options or of increasing numbers of states each had important but different consequences for the prospects of cooperation in the two situations. Further differences can be expected to occur within each of these two broad categories of collective action problems according to various contextual circumstances such as asymmetries among states or prospects for future play. These differences will be amplified by historical and other differences not directly accounted for in the game theoretic analysis. Moreover, these are only two of a much larger set of possible situations of interdependence and other possible strategic interrelations among states may have yet different implications for regimes.[27] All this suggests an enormous potential diversity of regimes. However, I concentrate on the diversity that arises from the two polar cases of coordination and assistance problems and the corresponding implications they hold for regime characteristics—especially as they pertain to questions of institutionalization and the stability (and adaptability) of different regime institutions. This approach demonstrates quite clearly how different problems of collective action may result in different regime solutions.

The most basic distinction between the two categories of regimes is that between contract and convention as forms of solutions to cooperation problems. This distinction is often treated as one between the need for centralized enforcement versus the possibility of decentralized outcomes. Although this is the case in many circumstances, it is not a hard and fast rule. As I have discussed, cooperative outcomes to PD sometimes can be achieved without centralized enforcement when a problem recurs through time; similarly, centralized institutions could provide the basis for conventional outcomes. Instead of being viewed as a defining characteristic of the two situations, the occurrence of centralized versus decentralized resolutions is better treated as a consequence of the underlying nature of solutions by contract and by convention. Contract is a solution to a particular type of cooperation problem of assistance (i.e., separable externalities) and is based on the mutual performance (or forbearance) of particular acts so as to benefit others. States surrender some of their authority or autonomy in return for other states doing the same. For example, states might agree to surrender the right to produce certain sorts of weapons or to impose certain restrictions on imports. Convention is a solution to a different problem of cooperation (i.e., non-separable externalities) and is based on the coordination or harmonization of policies among states. It does not require that states give up any autonomy per se although negotiation of coordination points will affect the choices they will want to make independently. For example, states might agree on target macroeconomic policies and for that reason find it in their self-interest to pursue those targets even though they are still free to pursue other targets. Thus contract and convention provide different categories of basic solutions to fundamentally different problems of interdependence. As Stein (1982, p. 316) points out, cooperation in such issues depends on "rational self-interested calculation (which) leads actors to abandon independent decision-making in favor of joint decision-making." But the underlying differences in the two situations result in important differences in the nature of joint decision making and hence in the nature of regimes.

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[27] As mentioned in note 7, a prominent case in point is "Chicken" which, for example, contains interesting problems of escalation not found in either PD or coordination.

Institutionalization

A first distinction concerns the nature of the basic rules and the extent of institutionalization (i.e., the nature of formal political arrangements) which characterize an issue area. In convention, rules will be largely self-enforcing and states will obey them (which is in the collective interest) because it is in their own self-interest. In contract, rules are not self-enforcing and there will be a tendency for them to be weakened by states cheating. The conflict between self-interest and collective interest persists even after rules are formulated. This difference between convention and contract will be reflected directly in the level and nature of institutionalization and, as mentioned above, PD regimes that rely on contract will likely be more centralized in order to regulate the actions of states within agreed-upon rules.

Regimes pertaining to coordination problems, and based on convention, will generally have low levels of institutionalization concerned primarily with facilitating the choice, interpretation, and observance of a particular convention. This will involve information gathering and informal consultation about the preferences and policies of states as well as providing a forum for the resolution of bargaining problems pertaining to the choice of a particular convention. Enforcement in the large will not be necessary, and exclusion does not make sense given the nature of the problem. (However, a subset of states may try to exclude others from the process of selecting a convention in order to impose one that corresponds more closely to their own preferences.) Some local enforcement may be desirable to prevent minor deviations from the convention in the small. States may be willing to accept this enforcement although adherence may be achieved through (negotiated) voluntary restraint since it entails only a small sacrifice to reinforce a convention producing much larger benefits.

Contract solutions to PD will likely require much higher levels of institutionalization in order to overcome the continuing incentives for states to defect from cooperative arrangements. This will be reflected in formal agreements restricting national behavior (i.e., akin to Hart's primary rules of law) as well as procedures to support these rules through centralized processes of adjudication (i.e., akin to Hart's secondary rules). Needless to say, the centralized powers of sanction are typically very limited and contract is sustained more by needs of maintaining ongoing cooperation and reputational considerations than through fear of any international sovereign. One notable exception to this is the sanction of exclusion from a regime. Threat of exclusion, if credible, may be an important device for ensuring that states behave cooperatively. The credibility of the threat will depend in part on the properties of the particular issue area (e.g., impossible for many international commons problems but not for trade issues) and in part on the nature of the political arrangements (e.g., a military alliance able to determine precisely the scope of its membership). If such exclusion is possible, then states may accept the authority of the international regime rather than face exclusion. More typical than the use of threats of expulsion against existing members is the requirement of acceptance of regime rules as a condition prior to admittance into the regime. This probably constitutes the single most important source of "centralized" enforcement.[28]

The second source of cooperation in assistance problems is the noncentralized enforcement that arises from reputational considerations and the prospects of playing through time. Indeed, one consequence of playing through time (or, alternatively, of the reaction time to each others' policies becoming exceedingly rapid) is that the strategic structure of the situation is transformed into one similar to that of the coordination game (Snidal, 1981; Taylor, 1976).[29] To the extent that these other incentives are adequate for maintaining cooperation, then solution by contract and centralized enforcement are not necessary. However, uncertainty about the possibility of maintaining cooperation over the long run, and the lack of trust among states coupled with continuing incentives to defect in any single play of the game (especially in issues involving large numbers of states), may make some form of centralized monitoring efficacious for maintaining cooperation. Therefore, even in this situation of repeated play through time, a greater level of enforcement is required (in PD) than in the pure coordination situation.

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[28] Sometimes countries are able to appeal to an outside third party to enforce an agreement. One example has been reliance on the superpowers to supervise regional agreements. This has been one important role for the United States in the Middle East over the past decade.

[29] This observation opens up important considerations for understanding how different solutions are possible for the same problem of collective action. Except for brief comments in the present and next paragraphs, I will not investigate that here. For a discussion of this, see Hardin's (1982) discussion of "contract by convention" as well as Snidal (1981).

However, reputations and considerations of ongoing cooperation are not restricted to single-issue areas but are important across issue areas (cf. Hardin, 1982, pp. 186-187). A horizontal proliferation of PD games may provide a more important incentive for cooperation than the vertical continuation of any single PD issue area through time. Fear that noncooperation will spread into other issue areas provide incentives for states not to succumb to immediate temptations to defect for short-run, issue-specific benefits. Indeed, this is a much more important aspect of issue-linkage than the more often discussed exchange form of linkage based on the linking of issues in a bargaining context. It is directly related to broader questions about the emergence of an international society with multiple overlapping interests and concerns. To the extent that such a society is emerging, the individual PD situation becomes embedded in a broader social context, and cooperation is increasingly possible with less formal centralized enforcement. In the extreme, variation between issues according to their strategic structure would tend to be dominated by the similarity imposed by these broader social considerations. But in less fully articulated international societies such as the present one, important regime variations will persist. Social considerations may be sufficient to enforce cooperation in PD issue areas of relatively minor importance where costs and risks of cooperation are low relative to benefits (e.g., international copyright law) but not in vital issue areas where they are high (e.g., military cooperation). Thus, in the current situation, successful resolution of major PD problems will require a higher level of institutionalization than in coordination problems.

Regime Stability and Transformation

Because a comprehensive international society is not fully developed, questions pertaining to the stability of regimes within particular issue areas are important. Sources of instability could include any of a wide range of changes in the circumstances or context of an issue area, but two potential sources of instability stand out as being of particular interest. The first is the stability of the regime in the face of underlying changes in the issue area itself.[30] The second is the stability of the regimes and their rules also to change. Second, issue power structure or, alternatively, increasing or decreasing asymmetry among states. But in analyzing each of these potential sources of instability one must be somewhat wary of the presumption that stability—even stability of cooperative outcomes—is always a virtue. First, changes in an issue area may affect the nature of desired cooperative outcomes so that it is desirable for regimes and their rules to also change. Second, different states may have different evaluations of the outcomes produced under various regimes, and those relatively least advantaged under particular regimes may welcome change. Of course in other cases the stability of cooperative outcomes will be more widely viewed as beneficial.

The striking feature of the coordination game is its stability in the single-play version, which is only slightly decreased when the situation recurs through time. If issue change is so substantial that the prevailing convention becomes dysfunctional (i.e., worse than no coordination) for regime members, then the regime will dissolve or transform. But for less dramatic issue change (which leaves the prevailing convention suboptimal but still better than no coordinating agreement whatsoever), the stability of the regime combined with likely disputes over which new convention to adopt will make it difficult to initiate a change to a better convention. This stability means that the regime will not always be sufficiently flexible in responding to new conditions which affect an issue area. This problem will be difficult to resolve because by their nature conventions cannot change quickly if they are to be effective. It can be mitigated through greater reliance on centralized institutions to adapt conventions to particular prevailing issue circumstances, but this negates many of the other advantages of conventional solutions which resulted from decentralized regimes. Alternatively, the existence of a single or small number of dominant actors may enable them to act as leaders in adapting new coordination points to changes in underlying conditions. However, except for this role of leadership, a decrease in the dominance of a single state is unlikely to be destabilizing to coordination outcomes. Therefore, coordination regimes are likely to be stable but, in the absence of leadership, may suffer from a lack of adaptability as issue circumstances change.

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[30] This sort of change must be limited to changes that occur within the basic strategic structure of the game. For example, an increase in the external military threat might increase the optimal level of security provision in an alliance or a change in economic conditions might require some common shift in economic policies. But changes so dramatic as to alter the underlying strategic structure cannot be analyzed in the same way.

The situation in PD is very different. The inherent instability of PD outcomes will be aggravated by changes in issue areas which affect the distribution of costs and benefits of cooperation—and perhaps even the precise definition of cooperative behavior by the individual state. Because it can be profitably abandoned by the individual state, success and stability of a regime depend on its ability to deliver benefits from cooperation relative to those available from alternative regime arrangements. This adjustment will be facilitated by the higher degree of institutionalization associated with contract regimes which enhances their capacity for adaptation and transformation. Centralized institutions will provide a framework for modifying the contract while a past history of cooperation will facilitate the emergence of new arrangements. But, especially with regard to larger adjustments, these transformations will be controversial since states have substantial conflict of interest in PD problems. Bargaining over the exact nature of new regimes will be intense and only well-established contract regimes will be able to orchestrate a successful transition without disruption of cooperation. The exception to this rule will be the special case of regimes concerned with public goods provision where leadership is provided by a single dominant actor.

Changes in the Distribution of Power among States

The impact of changes in the distribution of power among states can be usefully discussed in terms of the theory of hegemonic stability. The first (static) claim of the theory that the prospects for emergence of cooperative regimes are likely to be enhanced by asymmetries among states is essentially supported in both PD and coordination situations although it is subject to some refinement. In PD, the applicability of the generalization will depend heavily on the possibility for exclusion. If there is no exclusion, then we are in the public good case (which is the analytical underpinning of the theory) and the importance of dominant members to regime stability is explained by Olson's "privileged group" analylsis. Nevertheless, a problem of cooperation will remain because the dominant member will not provide the collectively optimal level of the good except in concert with other states. If exclusion is possible, then cooperation may be possible without a dominant state although such asymmetry will likely facilitate cooperation. In the coordination problem, conventions are likely to emerge without dominant states which are therefore not as important to cooperation as in PD. However, cooperation may be facilitated through the prominence of dominant states is establishing conventions—especially if changing circumstances require modification in the convention. Therefore, the claim of hegemonic stability theory about the role of dominant states in facilitating cooperation is largely valid, although it should not be over-stated to suggest that hegemony is necessary for, or always leads to cooperative outcomes.[31]

The second (static) claim that regimes based on the leadership of a dominant state will favor small states is not generally valid. The case where it does apply is again that of international public good provision where exclusion is impossible. But if exclusion is possible in a PD situation, then small countries will not have a free ride, and there is no reason to suppose that they will bear less than their fair share of the costs of cooperation through the regime. Finally, the claim is simply wrong in the coordination problem where regime outcomes will favor larger states which have a greater influence in determining coordination outcomes as discussed above.

Strictly speaking, a static model is inadequate to assess the dynamic claim that international regimes will emerge and decline in response to shifts in the concentration of power in an issue area. (The same applies to the derivation of hegemonic stability arguments themselves.) However, comparative statics analysis allows some observations about the reasonableness of the claim in terms of the preceding discussion of regime stability and transformation. Again, the theory seems plausible for the case its derivation is based upon, public goods, where the decline of a dominant power should decrease the regime's success in providing public goods until the emergence of a new dominant state restores the group's privileged status. In PD with exclusion or centralized enforcement, system stability will not be so dependent on the existence of a dominant state, and the regime may well be able to outlast shifts in the underlying issue power structure. In coordination problems, the decline of a dominant state need not herald the decline of the regime. The stability of the situation is such that the regime may persist indefinitely. Indeed, the most important role of dominant states is not that of preserving the status quo but of providing leadership in transforming the regime. Therefore, in coordination, the greatest challenge posed by the decline of a dominant state is not instability but the decreased ability of the regime to adapt to changes in the issue area.[32]

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[31] See Keohane (1984) and Snidal (1985) for further elaboration and qualification of this claim.

These dynamic considerations are quite speculative and a more precise specification of how states behave through time would be needed to obtain clearer predictions. However, even these crude speculations raise doubts concerning the general applicability of hegemonic stability theory and indicate the dangers of failing to distinguish between different problems of collective action and cooperation. Thus, the important point is not that the theory is right or wrong, but that it is inadequately specified to be taken as a general theoretical claim. Leadership in the international system is important, but it is important in different ways in different circumstances. The blessings of leadership are more mixed when viewed from the different perspectives raised by different problems of collective action.

Conclusion

These simple game models of PD and coordination are very spare and necessarily have severe limitations for the analysis of complicated real world issues. But even though they cannot capture all of the details and contextual nuances of particular issues, they provide valuable insights into the complexities posed by strategic interactions among states and how these affect international cooperation.

The analysis above has shown the great diversity of problems of international cooperation and the diversity of regime characteristics which is likely to result. The contrast between PD and coordination demonstrates that simple generalizations based on the PD model cannot be uncritically generalized to other situations. PD (and public goods) is an important problem, but it is not the singular problem of international cooperation, and other models such as coordination better characterize some important issues. These different strategic structures, and different contextual variation within strategic structures, have important consequences for regime characteristics ranging from the degree of centralized authority they require through their relationship to the distribution of global power and interest. Thus the analysis of the formation and development of international political regimes cannot be studied without an appropriate understanding of the strategic structure of the underlying issue area.

These same conclusions apply to the study of political organization and collective action more generally. Olson's (1965) public good analysis has been taken too literally as *the* problem of collective action. In fact there are many problems of collective action, and public goods are only one interesting category. Many other problems are PD situations involving purely private goods, while others, such as coordination, are even more fundamentally different. Recognizing these differences and coming to grips with them in various areas of political analysis will improve our understanding of the role of political organization in resolving collective action problems and help to explain the variety of institutional forms that such organization can take.

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[32] Further comparisons between PD and coordination with regard to their stability and adaptability can be made in terms of the distinction between "changes within regimes" and "changes of regimes" (Krasner, 1982a, pp. 187-188). Centralized institutions allow for a cleaner separation between fundamental principles and operating rules than do decentralized regimes, where the two may not be easily distinguished. Therefore centralized institutions will be better adapted to facilitating changes in regimes to deal with changes in issue circumstances without upsetting basic regime principles, and regime adaptability and stability may cease to be conflicting characteristics and become complementary.

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Conflict, War, and Redistribution

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This article analyzes the circumstances under which conflict leads to the outbreak of war using a formal model which incorporates both the redistribution of resources as an alternative to war and imperfect information. Countries act as rational agents concerned with both consumption and the public bad of a war. In the first period both countries can either consume or build arms, whereas in the second period there can be either the threat or the use of force to reallocate resources. If both countries are fully informed, then there will be no war but rather a voluntary redistribution of resources. In a situation of asymmetric information, however, in which one country is fully informed and the other is not, a war can occur if the uninformed country uses a separating equilibrium strategy, precommitting itself to a positive probability of war in order to prevent bluffing by the informed country.

When and under what circumstances does conflict between nations lead to the outbreak of war?[1] This fundamental question of international relations has been only partly answered by political theorists, and it leads to three other questions of clear significance. Can wars be explained as the result of rational behavior and thus be subject to analysis by social scientists?[2] Do arms races lead to the outbreak of war?[3] What is the role of threats and deterrence in prompting or preventing the outbreak of war?[4] Previous formal models have not fully answered these crucial questions, in part because they have not considered two factors relevant to outbreak of war: 1) the role of the redistribution of resources as a possible alternative to war, and 2) the role of imperfect information, in which nations may be uninformed about salient aspects of the problem. These two factors may lead to rational behavior that may involve risk taking, in which nations play a type of brinksmanship to gain or to avoid losing resources. The history of wars suggests that these factors have played important and, in some instances, crucial roles in explaining why wars did—or did not—occur.

The purpose of this article is to analyze the relations among conflict, war, and the distribution of wealth, using a formal model that incorporates these two factors to study the problem of the outbreak of war and related questions. We distinguish here between the threatened use of force in attempting to reallocate resources, which we call "conflict," and the actual use of weapons, which we call "war." The principal question is, when does conflict lead to the implementation of force and, hence, result in actual war, as opposed to the threat of war? An important issue treated in our model is the role of information, in particular whether asymmetric information can play a role in explaining the outbreak of war. The article also provides insights into circumstances in which an arms race can result in the outbreak of war. We show that in certain circumstances arms races do lead to war, whereas in other well-defined cases it can result in a stable equilibrium in which each side deters the other. Paradoxically, those equilibria in which wars do not occur are sometimes characterized by higher accumulations of weapons on both sides than those cases in which wars do occur. A key factor in distinguishing between these two cases is the initial distribution of resources, highlighting the influence of the distribution of wealth on stability as a guard against the outbreak of war. In fact, the formal model suggests that a redistribution of economic rights can serve as an equilibrating mechanism in international relations. Changing the distribution of

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[1] For a review of formal research on conflict theory, see Intriligator (1982).

[2] See Bueno de Mesquita (1981) and Organski and Kugler (1983).

[3] See Richardson (1951), Lambelet (1975), Wallace (1979, 1982), and Intriligator and Brito (1984).

[4] See Szilard (1964), Schelling (1966), and Intriligator (1975).

wealth to reflect the distribution of power can reduce the danger of outbreak of war. In the case of asymmetric information, however, in which one nation is informed but the other is not, the process of redistributing wealth may result in war because there exists the possibility that a country may try to bluff in order to increase its share of the resources in dispute. Such bluffs may be called, leading to war.

In this article we develop a formal model of countries acting as rational agents in which conflict, war, and redistribution may all occur. The countries are concerned with economic rights to consumption in a two-period model in which force can be used to redistribute these rights. The first period of the model represents an arms race in which countries choose between consumption and investment in arms. The second period is one of potential crisis as countries bargain and possibly use force or the threat of force in order to reallocate resources. This model represents further research into problems that we have addressed in the past, particularly in our work on the role of initial allocations in bargaining, on strategic stability via deterrence, and on rational models of arms races.[5] It builds on recent work on bargaining with asymmetric information[6] and uses some recent developments in models of self-selection[7] and of sequential equilibria.[8] In such models, two equilibrium concepts have been introduced. The first is that of a *separating equilibrium,* in which the uninformed agent attempts to induce the other party to reveal the pertinent characteristic; the second is that of a *pooling equilibrium,* in which the uninformed agent does not attempt to induce the other party to reveal the pertinent characteristic. In our model, agents are nation-states that can arm, redistribute resources voluntarily, or initiate a war, whereas the pertinent characteristic is each state's degree of aversion to war as measured by its tradeoff between the utility of consumption and the disutility of war.

Using this model, we show that if both countries are fully informed about the parameters of the problem, there will be no redistribution by actual war. Rather there will be a voluntary redistribution of resources, with neither side having an incentive to fight. War can occur, however, in a situation of asymmetric information, where one country is informed of all parameters of the problem, and the other is not informed about the parameter characterizing the first country's aversion to war (or willingness to fight). In such a situation, war can occur if the uninformed country adopts a separating equilibrium strategy, in which it precommits itself to a positive probability of war in order to prevent bluffing by the informed country. However, there would be no conflict if the uninformed country finds it optimal to adopt a pooling equilibrium strategy, in which it does not attempt to prevent such bluffing. There would also be no conflict if either country has very few resources and thus does not attempt to participate. The danger of war is greatest where the uninformed country has sufficient wealth to adopt a separating equilibrium strategy but not sufficient wealth to adopt a pooling equilibrium strategy, or when it has sufficient wealth to attempt to extract resources from the other country.

In addition to identifying situations in which the danger of war is greatest, the model can be used to address another question posed above, namely, whether arms races can lead to the outbreak of war. An arms race that leads to a separating equilibrium, in which threats are part of the process of deterrence, has a positive probability of resulting in war. All else being equal, such an outcome becomes less likely as the distribution of wealth approaches a certain stable set or as the probability that the opponent will bluff becomes smaller. In such cases, the uninformed agent would adopt a pooling equilibrium, which does not involve threats. The pooling equilibrium is sometimes characterized by a higher level of weapons than the separating equilibrium. In these cases, arms races that do not lead to war are characterized by higher levels of weapons expenditures than arms races that do lead to war.

In contrast to earlier models of arms races and conflict, such as the Richardson model,[9] existing game theoretic models[10] and expected utility models,[11] the present model highlights the importance of imperfect information and risk taking in explaining the outbreak of war. These factors, which are usually ignored in other formal theoretical models, appear to play a major role in the actual outbreak of war.

The next section summarizes some empirical regularities that have been discovered by researchers who have investigated the outbreak of war and that should be connected with a formal model of the outbreak of war. The third section then develops the basic model; the fourth treats the deterministic case in which both countries are fully informed; the fifth treats the asymmetric information case in which only one agent is fully informed. Conclusions and some potential extensions of the model and its applications are reported in the last section.

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[5] See Brito, Buoncristiani, and Intriligator (1977), Intriligator and Brito (1984), and Brito (1972). See also Intriligator and Brito (1976).

[6] See Nalebuff (1982), Cramton (1983), and Morton (1983).

[7] See Mirrlees (1971).

[8] See Kreps and Wilson (1982), Fudenberg and Tirole (1982), and Freixas, Guesnerie, and Tirole (1983).

[9] See Richardson (1960) and Intriligator and Brito (1976).

[10] See Schelling (1966) and Brams, Davis, and Straffin (1979).

[11] See Bueno de Mesquita (1981).

Empirical Studies of the Outbreak of War

Empirical studies of arms races and the outbreak of war have demonstrated certain regularities that should be consistent with, or explained by, formal theoretical models. The model to be presented in the next section is, in fact, consistent with these regularities and explains many of them.

Bueno de Mesquita (1981) used expected utility analysis to study the determinants of war. The details of this study have been controversial, but the study is persuasive in arguing that wars are consistent with rational decision making. Of the 58 wars that have been fought since the Congress of Vienna, the initiator won 42 of them. Bueno de Mesquita stated that if wars were nonrational events, then there should not be any systematic relationship between the initiator of a war and the victor, and he calculated that such an event should have occurred only once every 2,500 times. He then computed cost/benefit ratios and found that when wars occur, it was usually in the interest of one party. He did not, however, explain *why* wars occur. Wars are usually Pareto inferior outcomes of a conflict in that both parties would be better off if the expected loser compensated the expected victor by means of a transfer of resources without actually going to war. Thus, although the Bueno de Mesquita model demonstrates that wars are usually consistent with rational behavior on the part of the aggressor, it does not explain why wars actually occur, because it does not explain why the loser fought.

Wallace (1979, 1982) studied the outbreak of war using data on 99 serious great power disputes occurring since 1815. He was particularly concerned with whether or not arms races lead to the outbreak of war and concluded that conflicts and disputes accompanied by arms races are much more likely to result in war than those in which an arms race does not occur. Of the 26 conflicts and disputes that led to war, 23 involved an arms race, whereas of the 73 cases in which there was no war, 68 did not involve an arms race. Conversely, of the 28 cases in which there was an arms race, 23 resulted in war, whereas of the 71 cases in which there was no arms race, 68 resulted in no war. In addition, Wallace found that the probability of escalating to war depends on whether the "revisionist" power during the period before the crisis either wins or does not win the arms race. Of the 30 cases in which the revisionist power won, 12 resulted in war; of the 65 cases in which the revisionist power did not win, 12 resulted in war. These results suggest that there is an asymmetric relationship between the winner of the arms race and the probability that a crisis will escalate to war.

Siverson and Tennefoss (1983) studied the outbreak of war from 1816 to 1965. They divided conflict situations into three levels: threats, unreciprocated military action, and reciprocated military actions, and divided nations into four categories: allied major powers, unallied major powers, allied minor powers, and unallied minor powers. They found the following empirical regularities:

1. Among major powers, 61.7% of the conflicts never moved above the lowest level, and 19.8% of these escalated to the highest level.
2. When major powers initiated conflict with minor powers, 27% reached the highest level.
3. Minor powers initiated 19.1% of the conflicts with major powers, and 26.5% of these escalated to the highest level. However, there is no case in which an allied minor power initiated a conflict with a major power that reached the highest level.
4. When both countries had a major power ally, 58.3% of the conflicts remained at the lowest level.
5. If an allied major power initiated a conflict with an allied minor power, 16.7% went to the highest level and 26.8% remained at the lowest level.
6. If an allied major power initiated a conflict with an unallied minor power 42.8% of the cases went to the highest level and 26.8% remained at the lowest level.

Major powers and allied minor powers seem to be involved in less hostility than unallied minor powers, whereas conflict between major powers is limited. One possible explanation is that they are stronger and hence less likely to be attacked; however, this explanation is not consistent with the observations that unallied minor powers initiated 19.1% of the conflicts with major powers and that there is no case where an allied minor power initiated a conflict.

Smith (1980) attempted to identify arms races that are particularly war prone using statistical methods. She estimated the Richardson model and inferred stability from the estimated coefficients. In addition, she demonstrated that arms races do not always lead to war and showed that the probability of war could be predicted by the coefficients of the Richardson model. Smith did not explain what factors led to this result, but she noted the importance of risk factors and uncertainties that are not treated in the Richardson model and in subsequent deterministic models. However, these factors are treated in the model described below.

From this research, a pattern of the following empirical regularities appears to emerge: first, wars are usually consistent with rational decision making on the part of the initiator; second, an arms race is less likely to lead to war if the status-quo power "loses" the arms race; third, conflict between major powers is limited and is less likely to escalate into war than conflict between major and minor powers; fourth, major powers and allied minor powers seem to be involved in less hostility than unallied minor powers; and fifth, unallied minor powers initiated conflicts with major powers, but there is no case where an allied minor power initiated conflict. None of these results is yet carved in stone; however, they do form a reasonable starting point for a set of stylized facts that should be consistent with or explained by formal models of arms races and outbreak of war. We now develop such a model.

A Two-Period Model

This section introduces a two-period model in which there are two countries in conflict for the rights to a flow of a single, homogeneous good. For example, they may be in contention for a territory that yields a fungible resource such as oil, or the right to a market that yields income. In the first period, this good may be consumed or used to produce weapons, as in the usual guns-or-butter tradeoff. This process is irreversible, so weapons cannot be converted back into goods during the second period. In the second period, the distribution of the good can be altered either by war or negotiation. The outcome of war depends on the amount of fighting engaged in by either country, which in turn is limited by its weapon stocks as determined by its choices during the first period. War produces an externality which is a public bad and offsets the utility of consumption in the second period. Countries are assumed to be rational and nonaltruistic, and they will choose to fight if the expected value of such a strategy is greater than that of not fighting. If they do not fight, then they can negotiate a redistribution of resources in the second period. Each country is informed about all the parameters of the problem other than possibly the degree to which the war externality enters in the objective function of the other country. The fourth section treats the fully informed case in which the value of this parameter is common knowledge for both countries, and the fifth section treats the asymmetric case where only one country is fully informed.

We assume that each country has a utility function of the form

$$u\left[x(i), y(i), \frac{C}{n(i)}\right] = a[x(i)] + a[y(i)] - \frac{C}{n(i)}, \quad i = 1, 2. \tag{1}$$

Here $x(i)$ is first period consumption for the i^{th} country; $a[.]$ is a strictly concave utility of consumption function such that $a'[.] > 0$ and $a'[0]$ is finite;[12] $y(i)$ is second-period consumption; C is public bad that is produced by war and that reduces utility, such as nuclear fallout or the loss of trade owing to wartime disruption of commerce; and $n(i)$ is a parameter that determines the disutility of war to the i^{th} country. For simplicity, utility is assumed to be separable and linear in the externality. (This assumption could be easily generalized.) The value of the parameter $n(i)$ may or may not be known to the j^{th} country; however, if not known, we assume that each country believes that the parameter characterizing the other country's disutility of war takes one of two values: $n(i) = n_1$ with probability p or $n(i) = n_2$ with probability $(1-p)$.[13] Without loss of generality $n_1 < n_2$, so the disutility of war is greater for an n_1 country than it is for an n_2 country as measured by the amount of utility of consumption needed to offset a particular level of the externality.

In the first period, each country faces a resource constraint of the form

$$K(i) = x(i) + w(i) \tag{2}$$

where $K(i)$ represents the total goods available to the i^{th} country in the first period and $w(i)$ is the

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[12] This assumption is made so that the game is such that an agent has the option of not participating. The utility level of $a(0)$ can be thought of as the utility of staying home as opposed to engaging in adventures abroad in order to acquire a right to a share of total goods available K, which, in turn, could be thought of as representing gains from international trade and diplomacy.

[13] This particular basis for uncertainty is not crucial to the properties of the model. For example, we could have modeled the problem as one of uncertainty in the technology of conflict, where the j^{th} agent is uncertain as to the i^{th} agent's effectiveness. What is crucial is that there is uncertainty for the j^{th} agent about the cost or outcome of conflict or both. Also we could have formulated the uncertainty in other ways, for example, a continuous distribution over the parameter $n(i)$ reflecting the beliefs of agent j.

i^{th} country's investment in weapons. It is assumed that total resources $K(i) + K(j) = K$ are fixed; thus, $K(i)$ is sufficient to describe the allocation of wealth.

By the second period, both countries have chosen weapon stocks $w(i)$ and $w(j)$ respectively, and there are then three possibilities. First, both countries could choose to behave passively and preserve the status quo. Second, one country could propose a redistribution agreed to by the second country. Third, one or both countries could propose a redistribution not agreed to by the other side that could lead to conflict and possibly to the outbreak of war.

We define as the *war allocation* the distribution of resources and the externality that result from the war described by the game

$$C = v(i) + v(j) \tag{3}$$

$$Y(i) = K(i) + f[v(i), v(j)] \tag{4}$$

$$v(i) < w(i) \tag{5}$$

$$v(j) < w(j). \tag{6}$$

Here $v(i)$ is the amount of fighting by the i^{th} country, as measured by the amount of weapons the country commits to the war. It is assumed that committing more weapons shifts the redistribution, measured in equation (4) by $f[v(i), v(j)]$, in that country's favor and that[14]

$$\frac{\partial f}{\partial v(i)} > 0, \ \frac{\partial f}{\partial v(j)} < 0.$$

Note that the amount of fighting a country is able to do is limited by its level of weapons, which was determined in part by the initial distribution of resources in the first period and that the redistribution is from one to the other so

$$f[v(i), v(j)] = -f[v(j), v(i)].$$

The value of this game for possible values of the parameters $n(i)$ together with the proposed redistribution define a game that will have an equilibrium in mixed strategies.

Setting aside for the moment the second possibility of a proposed redistribution, the complete game can now be described. In the first period, each country chooses between goods and investment in weapons. This investment has two implications: first, it provides the countries with the ability to fight in the second period; and, second, it may reveal information about the value of parameter $n(i)$.

Countries can choose two possible strategies for investing in weapons. First, they can adopt a *war arms investment strategy* in which they invest in weapons as if they planned to engage in fighting in the second period. Second, they can adopt a *deterrence arms investment strategy* in which they invest in a sufficient level of weapons in order to deter the other country from initiating a war in the second period.

Because our model is very different from models that traditionally have been used to address similar problems, it would be useful to discuss the assumptions that we have made. A basic assumption is that of rational agents maximizing utility. Another basic assumption is that of a two-period model; such models are routine in economics but have not, to our knowledge, been previously applied in political science or international relations. These two assumptions probably bias the model in the direction of no outbreak of war. The assumption of rationality leads to agents adopting a cooperative solution. It has been very difficult to develop models in which rational agents carry out threats ex-post. The assumption of two time periods precludes early belligerent behavior to establish a reputation for toughness. The special assumptions of the model, specifically that there exists a mechanism for reallocating resources and that war produces a negative externality that affects the countries in different ways, appear to be reasonable ones that are consistent with the reality of conflict between nations. The technical assumptions of differentiability, continuity, and compactness are relatively harmless, and most formal models in international relations make even stronger assumptions in which these properties are implicit.[15]

The Fully Informed Case

This section treats the case in which each country is fully informed and knows the utility function of the other country, including $n(i)$, so that there is no uncertainty in the model. In this case, each country maximizes utility, leading to reaction functions that can be interpreted as the discrete time analogs of the Richardson equations. An equilibrium level of weapons that is consistent with these reaction functions exists in pure strategies when at least one country arms for war and in mixed strategies when both countries arm to deter war. There will always be feasible redistributions of resources in the second period that will be preferred by both countries to the outcome resulting from a war between them.

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[14] The redistribution function $f[v(i), v(j)]$ plays a role similar to that of the threat-point in an earlier work on the Nash bargaining problem, in Brito, Buoncristiani, and Intriligator (1977).

[15] The mathematical assumptions we have made are sufficient for us to use the Brouwer and Kakutani fixed-point theorem and the Berge maximum theorem. Most of the arguments in the article involve existence and continuity properties of the solutions. Marginal conditions play only a background role in the argument, and, in general, they will not be exhibited. The reader can work out the necessary Kuhn-Tucker conditions on the particular properties of the maximization. Upon reflection, it is not surprising that marginal conditions are not very interesting in this context. The decision to fight or not to fight is not a marginal one.

If the countries adopt a war arms investment strategy, the i^{th} country chooses $w(i)$ in the first period and, in the second period, maximizes, by choice of its level of fighting $v(i)$, its second period utility, given as

$$a[y(i)] + \frac{C}{n(i)} \tag{7}$$

subject to equations (3) to (6), where it is assumed that $v(i)$ is chosen rationally by the j^{th} country in response to the i^{th} country's actions. Defining $V[w(i), w(j), n(i)]$ as the solution to the i^{th} country's problem in the second period leads to

Proposition 1. If $w(i) > 0$, then $\frac{\partial V}{\partial w(i)} > 0$,

and thus the resource constraint defined by equation (2) is always binding.

The proof of this proposition as well as all other formal proofs are in the appendix of this article. The proposition implies that the i^{th} country will not acquire weapons it would not be willing to use if there is a war. Because the proposition holds for both countries, it is possible to solve for reaction functions of both. The first period resource constraint (equation (2)) can be used to solve for first-period consumption $x(i)$. The i^{th} country's first period maximization problem can then be written

$$\max a[K(i) - w(i)] + V[w(i), w(j), n(i)], \tag{8}$$

and a similar problem exists for the j^{th} country.

The solution to these problems are the reaction functions

$$w(i) = \phi\,[w(j), n(i), K(i)],\ i, j = 1, 2, \tag{9}$$

showing the reaction of country i to country j. The two equations defined by system (9) describe the dynamics of the arms race and are the discrete time analogs of the Richardson equations in that they describe the dynamics of the arms race. Equations (9) and the Richardson equations have similar properties in that an equilibrium exists and depends on the economic resources of the countries involved. Formally, it is possible to state two theorems.

Theorem 1. For any given initial allocation, $K(i)$, there exists an equilibrium weapons allocation that is consistent with the reaction functions given by equation (9).

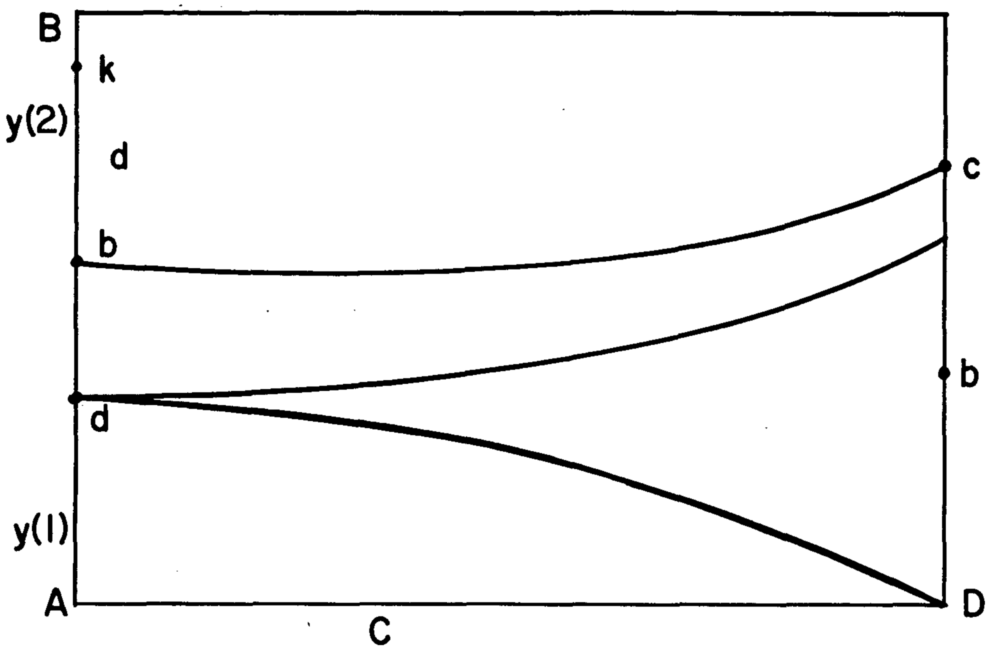
Theorem 2. There exists a set of initial allocations that are strictly preferred to the war allocation by both countries.

Some of the properties of this game can be illustrated in Figure 1, where the vertical axis represents second-period consumption for both countries and the horizontal axis is the level of the externality caused by war. The private consumption of country 1 is measured upward from point *A*, and the private consumption of country 2 is measured downward from point *B*. Country 1 has indifference curves that slope upward since *C* is a bad; similarly, country 2 has indifference curves that slope downward. The length of the line *AB* is total resources, $K(1) + K(2)$, and the length of the horizontal axis *AD* is total weapons available, $w(1) + w(2)$ (as in the Edgeworth box diagram). Let the initial allocation be at point *a*. In the second period there are two possibilities: the war allocation could either be at a point like *b* or it could be at a point like *c*.

If the war allocation is at a point like *b*, then war will not occur in the second period since both countries prefer the allocation at point *a* to the allocation of point *b*. Having shown that points like *b* exist, points on the axis in the neighborhood of point *a* illustrate Theorem 1 representing equilibrium allocations. If, however, the war allocation is at a point like *c* relative to the initial allocation at point *a*, then country 1 would prefer the allocation at point *c* and thus would want to initiate a war. However, both countries would prefer an allocation like *d* that is feasible and dominates *c*. Rather than fight, country 2 should rationally negotiate and reach a settlement in the interval (*k*, *h*), that is the set of points that dominate *c* for both countries. If such voluntary transfers are possible, then the conflict arms investment strategy is not consistent with rationality in this certainty model. This is a major logical difficulty with the war arms investment strategy in which all parameters are known with certainty.

The case in which voluntary transfers are possible is not the only one in which there are difficulties with the war arms strategy in a certainty model. Consider the case where the war allocation is at point *b*. That point is strictly not preferred to the allocation at point *a* by either country. Thus, either country can improve its welfare by increasing consumption in the first period as long as the resulting war allocation is not preferred by the other country. In such a case, unilateral action

Figure 1. War Allocation and Initial Allocation in the Fully Informed Case



can improve the outcome for a country, so the war arms investment strategy is not a Nash solution if the possibility of not fighting is allowed. Note in particular that in the case we are illustrating both countries have the same levels of consumption at *a* as at *b*, but that at *a* there is no war, and hence both are better off at this point.

An alternative to the war arms investment strategy is a deterrence arms investment strategy. Defining $T(i)$ as a voluntary transfer from the i^{th} country to the j^{th} country in the second period, an alternate way to formulate the problem is for the i^{th} country to maximize total utility of consumption over both periods

$$a[x(i)] + a[y(i)] \qquad (10)$$

subject to the incentive constraint that country j be no worse off after the transfer than the utility level it could have achieved by war, namely

$$a[K(j) + T(i)] > a[K(j) + f[w(i), w(j)]] - \frac{[w(i) + w(j)]}{n(j)} \qquad (11)$$

for a given $w(j)$ as well as the resource constraints for i in both periods.

$$K(i) - w(i) - x(i) = 0 \qquad (12)$$

$$K(i) - T(i) - y(i) = 0. \qquad (13)$$

The incentive constraint requires that the i^{th} country define the feasible set of the j^{th} country such that, in equation (11) country j weakly prefers the nonwar allocation to the war allocation.[16] It is similar to the "self-selection constraint" in the optimal tax literature, for example, in Mirrlees (1971).

The solution to this problem represents the maximum consumption the i^{th} country can achieve while guaranteeing that it would not be optimal for the j^{th} country to engage in war given that it has $w(j)$ weapons. The solution of this optimization defines the mapping

$$w(i) = \psi\,[w(j), n(j), K(i)]. \qquad (14)$$

This reaction function for the i^{th} country,

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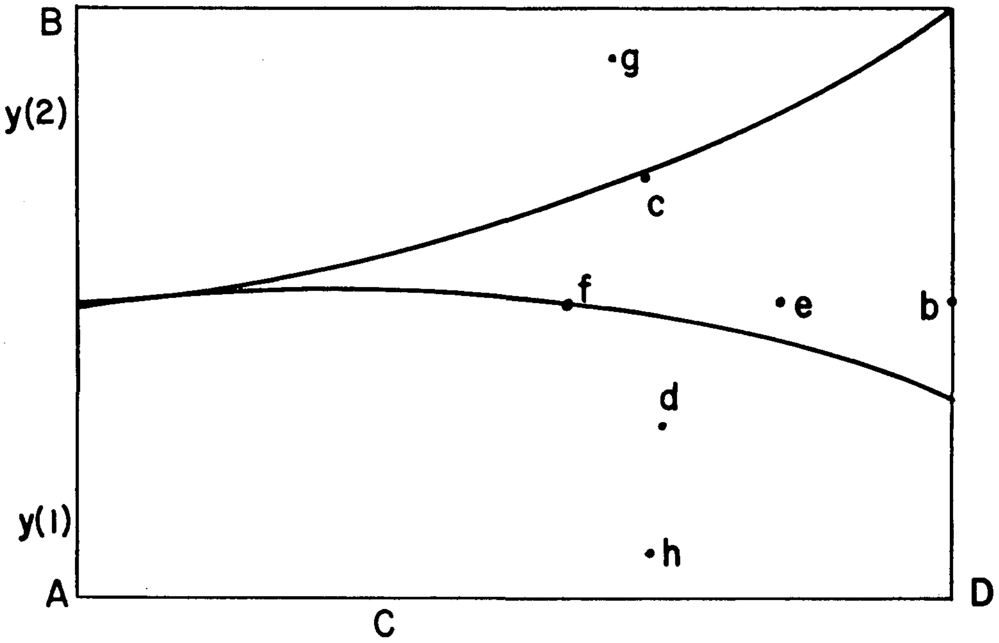
[16] We realize that we are avoiding the problem of determining how the surplus created by not fighting is distributed in a bargaining situation. For the sake of simplicity, we are assigning the entire surplus to the party making the transfer. Clearly any rule from axiomatic bargaining theory could also be used.

Figure 2. Possible Equilibria in the Fully Informed Case



together with the reaction function given by equation (9) for the j^{th} country, defines the arms race for the case where one country adopts a deterrence arms investment strategy and the other country adopts a war arms investment strategy. Then:

Theorem 3. If the transfer $T(i) > 0$, an equilibrium level of weapons exists and the allocation that results from the i^{th} country adopting a deterrence arms investment strategy and the j^{th} country adopting a war arms investment strategy is a Nash equilibrium.

It should be stressed that the solution for the i^{th} country strategy depends on $n(i)$, its own parameter, if that country is adopting a war investment strategy, and on $n(j)$, the other country's parameter, if it is adopting a deterrence investment strategy. This point is important in understanding why deterrence may lead to higher arms levels. If a country believes that the other country is tough, it may feel that it is necessary to invest in high weapons levels in order to deter. If, on the other hand, the country is adopting a war investment strategy, it will choose a level of weapons—and thus a level of fighting—at which the marginal benefits from fighting are equated to the marginal costs from fighting. Thus, if $n(j) > n(i)$ and if $T(i) > 0$, then the i^{th} country may invest in a *greater* level of armaments if it adopts the deterrence arms investment strategy than it would have if it had adopted the war arms investment strategy. This apparent anomaly results from the fact that in the deterrence arms investment strategy, the level of arms acquired by the i^{th} country is dependent of $n(i)$, although it does depend on $n(j)$. Thus, if $n(j)$ is much greater than $n(i)$, the deterrence arms investment strategy will have an equilibrium with a higher level of arms than the war arms investment strategy.[17]

Theorem 4. If $T(i) = 0$, and if both countries adopt a deterrence arms investment strategy, then the resulting arms investment strategy is not a pure Nash equilibrium.

The process of both countries reducing weapons could lead to war. Let us continue the process one more stage. If one side reduces weapons further, given that the other side is adopting a deterrence strategy, the first side could do better by adopting a war arms investment strategy given the low weapons level of the other side. Possible equilibria are depicted in Figure 2 using the same diagram as before. Point *b* is the allocation associated with the war arms investment strategy for both countries, whereas points *c* and *d* are the allocations that would result if one country adopts the war arms investment strategy and the other adopts a deterrence arms investment strategy. For example, at *c*, country 1 adopts a war arms investment strategy, while country 2 adopts a deterrence arms investment strategy. Point *e* is the allocation that results if both countries adopt the deterrence arms investment strategy, each assuming that the other is using a war arms investment strategy. This point illustrates Theorem 4 in that it is not a Nash equilibrium; at this point, each country would prefer to reduce its level of weapons. Although *e* is not a Nash equilibrium, it is a saddle point for this nonzero-sum game.

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[17] A similar anomaly appears in Intriligator (1975), where, given that the other side has very few weapons, it requires more weapons to deter the opponent than to attack it.

If both countries were to adopt a deterrence arms investment strategy, the resulting allocation would be a point like *f*. This point, however, is unstable, because if either country observed such a point, it would find it advantageous to switch to a war arms investment strategy, as illustrated by points *g* and *h*. For example, country 1 strictly prefers point *g* to point *c* and thus to point *a* (which is on the same indifference curve as point *c*). Because *a* is strictly preferred to *f*, it follows that *g* is preferred to *f*, so country 1 has an incentive to switch from a deterrence to a war arms investment strategy. The levels of weapons associated with point *e* are the minimum numbers required to ensure that it is not in the self-interest of either country to use force to redistribute.[18]

Theorem 5. There is a Nash equilibrium in the space of mixed strategies.

The mixed strategies used by the countries involve probabilities of investing in weapons at different levels, thus having the possibility of realizations that may result in a reallocation of resources.

If nations are major powers or are allied to major powers, then it is more likely that conflict will involve countries with a knowledge of the other as they will have had a history of mutual interaction. Thus there is less need for using war as a screening mechanism to determine the characteristics of the other country. Conflict then may result from the realization on mixed strategies, but among rational countries this should not lead to military action. This result is consistent with one of the empirical regularities noted by Siverson and Tennefoss—that major-power conflicts and conflicts that involve allied minor powers tend to be limited to lower levels.

Asymmetric Information

This section treats the asymmetric information case in which only one country knows the utility function of the other. Although this is not the most general case, it is nonetheless an important one. It is not unheard of for established powers, particularly major powers, to test the determination of new governments, particularly minor powers, in strategic confrontations. As another example, there are important asymmetries in information concerning closed vs. open societies.

In this case, both informed and uninformed countries maximize expected utility. The informed country has the option of bluffing in order to increase the transfer made to it by the uninformed country. The uninformed country may call the bluff by offering the informed country a choice that involves a nonzero probability of war (a separating equilibrium). Alternatively, it can simply assume that the other country is not bluffing and not run the risk of war (a pooling equilibrium). Whatever equilibrium is chosen depends on the relative wealth of the two countries and the underlying distribution of types. If either country is very weak, or if the distribution of wealth falls within a certain critical range (the stable set), then there is a zero probability. In the first instance, the poor country cannot afford to arm for war; in the second case, the pooling equilibrium dominates the separating equilibrium. A separating equilibrium, with a positive probability of war, may exist between these two extremes. These results are consistent with the empirical regularities discussed above.

Without loss of generality, it will be assumed that the j^{th} country is informed and knows the value of the war aversion parameter of the i^{th} country, which knows only that $n(j) = n_1$ with probability p and $n(j) = n_2$ with probability $(1-p)$, as introduced above. The i^{th} (uninformed) country's first period maximization problem for the war investment strategy can then be written as one of expected utility maximization

$$\begin{aligned}\max p\{a[K(i) - w(i)] + V[w(i), w(n_1), n(i)]\}\\ + (1-p)\,\{a[K(i) - w(i)]]\\ + V[w(i), w(n_2), n(i)]\},\end{aligned} \tag{15}$$

where $w(n_1)$ is the level of weapons adopted by the j^{th} country if it is of type n_1, and where $w(n_2)$ is the level adopted by the j^{th} country if it is of type n_2. Let the reaction function

$$w(i) = \alpha\ [w(n_1), w(n_2); p, n(i), K(i)] \tag{16}$$

be the solution to this maximization problem. The

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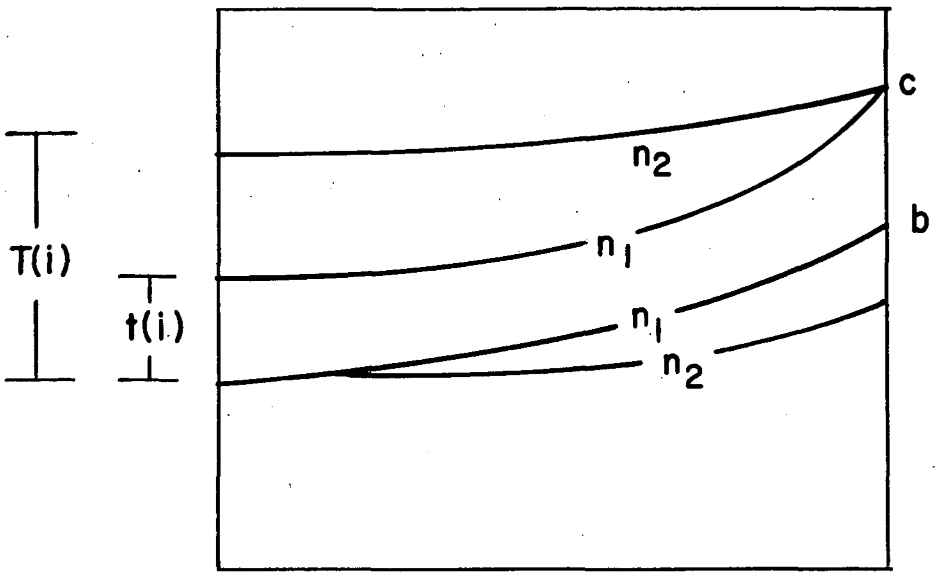
[18] Point *e* corresponds to the minimum number of weapons required for mutual deterrence, that is, the minimum point in the cone of mutual deterrence in Intriligator (1975).

Figure 3. War Investment Strategy Allocation and the Status Quo in the Asymmetric Information Case



j^{th} (informed) country's problem can then be written

$$\max a[K(j) - w(j)] + V[w(j), w(i), n(j)], \quad (17)$$

as in equation (8). The solution to this maximization problem yields the reaction function

$$w(j) = \beta\ [w(i);\ n(i),\ n(j),\ K(i)],$$
$$n(j) = n_1, n_2. \quad (18)$$

The fixed point defined by these reaction functions is the war investment strategy if the j^{th} country does not behave in a strategic manner. It may, however, pay the j^{th} country to behave strategically. In Figure 3, we can see that the war investment strategy allocation at a point like b would be preferred to the status quo at point a by the j^{th} country if it were of type n_2, but not if it were of type n_1. Thus, if the i^{th} country adopted the deterrence arms investment strategy, the j^{th} country would receive a positive transfer if it were of type n_2 but nothing if it were of type n_1. The uninformed country's problem is to pick both a level of investment in weapons in the first period and a transfer in the second period that will maximize its expected utility, given its prior belief p concerning the informed country's utility function and its wealth $K(i)$.

Define $w_d(n_1)$ as the optimal weapons level acquired by an n_1 country if it does not attempt to bluff to make the i^{th} country believe it is of type n_2. Define $w_d(n_2)$ similarly as the optimal weapons level acquired by an n_2 country. The i^{th} country wants to induce the j^{th} country not to fight, and it will attempt to do so by offering a transfer to it of $T(i)$, increasing the country's resources in the second period to $K(j) + T(i)$. There is a probability p, however, that the j^{th} country may bluff, claiming to be of type n_2 in order to receive this transfer.

To prevent this type of bluffing behavior by the j^{th} country, the i^{th} country will choose a probability q of fighting and a probability $1-q$ of making the transfer $T(i)$. If the conflict allocation is at a point like c, then the lottery would involve a transfer of $T(i)$ to an n_2 country and a transfer of $t(i) < T(i)$ to an n_1 country.

The i^{th} country, knowing that $w_d(n_2)$ is optimal if the j^{th} country is of type n_2, will propose a lottery such that it will give the j^{th} country $T(i)$ with probability $1-q$ and fight with probability q if the j^{th} country chooses $w_d(n_2)$ and will give no transfer if the j^{th} country chooses $w_d(n_1)$. This lottery is chosen so that if the j^{th} country is of type n_2 it will choose the lottery, whereas if the j^{th} country is of type n_1 it will optimally choose $w_d(n_1)$.

The choice of the lottery, with a probability q of fighting, is intended to prevent bluffing by the opponent, whereas the choice of the weapons level is intended to deter the opponent.[19] The equilibrium obtained if the i^{th} (uninformed) country uses this strategy is called a *separating equilibrium,* because it prevents bluffing by separating the n_1 from n_2 countries. Given this strategy, the j^{th} country will choose $w_d(n_1)$ if it is of type n_1 and $w_d(n_2)$ if it is of type n_2.[20] An alternative equilibrium that has been treated in the literature on sequential games is a *pooling equilibrium* where the j^{th} country will choose $w_d(n_2)$ regardless of whether it is of type n_1 or n_2. Country i will choose a strategy leading to a pooling equilibrium if the costs of discriminating between the two types of countries are too high relative to the potential gains from such a separating equilibrium.

If war occurs, the outcome for the i^{th} country is defined as

$$V[w_d(i), w_d(j), n(i)] = a[K(i) + f[w_d(i), w_d(j)]] - \frac{[w_d(i) + w_d(j)]}{n(i)} \tag{19}$$

Define $V(1)$ to be the outcome of war for the j^{th} country if $n(i) = n_1$ in equation (19) and $V(2)$ to be the outcome of war if $n(i) = n_2$. If the i^{th} country wants to solve for the separating equilibrium, it maximizes expected utility, given as

$$a[x(i)] + p\{a[K(i)]\} + (1-p)\{qV[i] + (1-q)a[[K(i) - T(i)]]\}, \tag{20}$$

that is, the utility of first-period consumption plus the probability of consuming $K(i)$ if the other country is of type n_1, plus the probability of the lottery involving either conflict with probability q or transfers with probability $1-q$. This maximization is subject to the incentive constraint that if country j is of type n_2 then it is indifferent between redistribution $T(i)$ and the outcome of a conflict

$$a[K(j) + T(i)] - V[2] = 0, \tag{21}$$

whereas if country j is of type n_1 then it is indifferent between the initial allocation and the lottery

$$a[K(j)] - \{qa[K(j) + T(i) + (1-q)V[1]\} = 0. \tag{22}$$

The maximization is also subject to the resource constraint

$$K(i) - w_d(i) - x(i) = 0. \tag{23}$$

Let Ψ $[K(i), n_1, n_2, p]$ be the value of the separating equilibrium solution of the problem.

If the i^{th} country wants to compute a pooling equilibrium, it maximizes

$$a[x(i)] + a[K(i) - T(i)] \tag{24}$$

subject to the same incentive constraint as equation (21) and the resource constraint (23). Let π $[K(i), n_1, n_2]$ be the value of the pooling equilibrium solution of the problem.

The difference between the values of the separating and pooling equilibrium is

$$\Delta \equiv \Delta[K(i), n_1, n_2, p] = \Psi[K(i), n_1, n_2, p] - \pi[K(i), n_1, n_2]. \tag{25}$$

If $\Delta > 0$, then the i^{th} country prefers a separating equilibrium to a pooling equilibrium, whereas if $\Delta < 0$, the i^{th} country prefers a pooling equilibrium to a separating equilibrium.

Theorem 6.

$$\frac{d\Delta}{dp} = a[K(i)] - \{qV(i) + (1-q)a[K(i) - T(i)]\} > 0. \tag{26}$$

So the relative value of a separating equilibrium increases as the probability that the other agent is of type n_1 increases.

It should be noted that it is possible that the separating equilibrium may never dominate the pooling equilibrium. Examples can be constructed by scaling the amount of the externality created by the conflict such that for all $K(i)$ the conflict solution is dominated by the status quo for both countries regardless of type. In that case, the separating equilibrium would involve no transfers for all $K(i)$ and would thus trivially be a pooling equilibrium. It is possible, however, to show that there always exists a set with positive measure such that the pooling equilibrium will dominate the separat-

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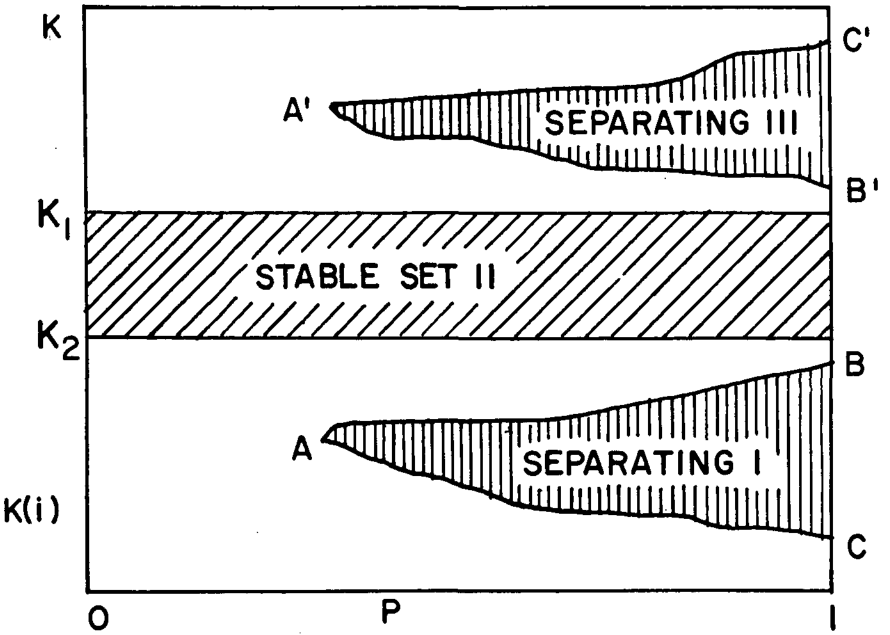
[19] The idea that threats could be used as a screening mechanism were to our knowledge first used by Sanford Morton in a 1979 informal working paper at Carnegie-Mellon University, which later appeared in a revised form in Morton (1983), and independently by Beth Hayes in her thesis at the University of Pennsylvania.

[20] We realize that a separating equilibrium requires precommitment on the part of the i^{th} agent and thus violates the "perfectness" of the equilibrium. For a discussion of perfectness see Kreps and Wilson (1982). We believe, however, that precommitment is not just a theoretical problem but is one that occurs in the real world. For example, there are practical alternatives to the deployment of intermediate-range nuclear missiles in Europe, but these alternatives do not precommit the United States to defend Europe.

Figure 4. Pooling and Separating Equilibria



ing equilibrium for all values of p. This implies that there exist distributions of wealth that are stable and that will not lead to war.

Theorem 7. There exists an interval of positive measure, $[K_1, K_2]$, the stable set, in the interval $[0, K]$ such that the pooling equilibrium will dominate the separating equilibrium for all values of p.

Theorems 6 and 7 enable us to characterize the configuration of possible equilibria. We know from Theorem 7 that p in the stable set results in a pooling equilibrium. Further, for all $K(i)$, the locus of points such that $p = 0$ is such that $\Delta < 0$. Thus, if there exist regions in Γ where $\Delta > 0$, the configuration of equilibria is represented as in Figure 4. (Note that the variable on the horizontal axis is p here rather than C.) Examining the locus of points such that

$$\Delta [K(i), n_1, n_2, p] = 0, \tag{27}$$

we find that the boundary between the pooling and separating equilibria is the locus of points illustrated as A-B-C in Figure 4. Points above the A-B locus involve a pooling equilibrium, whereas those below this locus involve a separating equilibrium.

Because $\Delta [K(i), n_1, n_2, 0] < 0$, the point A must lie in the strict interior of the set Γ. Theorem 6 implies that the boundaries of A-B and A-C must be monotonic.

Figure 4 also shows a locus A-C below which there is no game. If country i has very few resources $K(i)$, then it will allocate all of its resources to first-period consumption, leaving nothing for weapons accumulation and thus entailing no conflict with country j. Such a country is, in effect, not wealthy enough to engage in conflict. The locus A-C is downward sloping, because as the opponent has a higher probability of the smaller parameter n_1, the expected value of participating in the game in the second period increases owing to a lower expected cost of deterrence. The points A', B', and C' refer to comparable points except that the i^{th} country is extracting transfers from the j^{th} country.

For most fixed levels of p, Figure 4 shows that an increase in the i^{th} country's resources as measured by $K(i)$ may lead to three distinct types of equilibria. The first, for low values of $K(i)$, is one in which country i does not accumulate weapons at all. The second, for intermediate levels of $K(i)$, is one of a separating equilibrium in which country i accumulates weapons and uses them to deter country j in such a way that there is a probability that war will occur. The third, for levels of $K(i)$ in the stable set, is one of a pooling equilibrium in which country i accumulates weapons but has enough weapons so that there is a zero probability that war will occur. At yet higher levels of $K(i)$, the i^{th} country has enough weapons so that it can extract resources from the j^{th}. Finally, when $K(i)$ is large enough, country j does not accumulate weapons at all because $K(j)$ is so small. It is important to note that transfers are from the weak to the strong, the strong nation extracting transfers from the weaker nation.

An unallied minor power is more likely to be uninformed about the characteristics of the other country or, alternatively, that other countries will be uninformed about its own characteristics. Thus, it is more likely to be involved in situations that result in separating equilibria than established participants. Region I in Figure 4 characterizes the situation in which a minor power would initiate conflict with a major power, whereas Region III characterizes a situation in which a major power would initiate conflict with a minor power. Thus, this theory is consistent with the Siverson-Tennefoss observations that unallied minor powers initiate conflict with major powers with a relatively high frequency.

Conclusions and Potential Extensions

We have developed in this article a formal model in which conflict, war, and redistribution may occur. Countries are assumed to behave as rational actors concerned with economic rights to consumption within a two-period model in which force can be used to redistribute these rights.

Using this model, we have shown that if both countries are fully informed about the parameters of the problem, there will be no redistribution by actual war; rather, there will be a voluntary redistribution of resources, and neither side will have an incentive to fight.

We have also shown that war can occur, given the structure of this model, in a situation of asymmetric information—where one agent is informed of all parameters of the problem while the other is not informed about the parameter characterizing the utility function of the first agent. In such a situation, war can occur if the uninformed agent adopts a separating equilibrium strategy—in which it precommits itself to a positive probability of war in order to prevent bluffing by the informed agent. However, there would be no war if the uninformed agent finds it optimal to adopt a pooling equilibrium strategy, in which it does not attempt to prevent bluffing. This can occur if the distribution of resources is such that it is not in the interest of either country to engage in conflict or if the uninformed country has enough resources to bribe the other country via a transfer of part of its wealth. There would also be no war if either agent has very few resources. The probability of an actual war outcome is greatest when the uninformed agent has sufficient wealth to adopt a separating equilibrium strategy but not sufficient wealth to adopt a pooling equilibrium strategy or when it has sufficient wealth to attempt to extract resources from the other.

These results can be used to address the question posed in the introduction of whether arms races can lead to the outbreak of war. An arms race that leads to a separating equilibrium—in which threats are part of the process of deterrence—has a positive probability of resulting in war. Other things being equal, such an outcome becomes less likely as the distribution of wealth approaches the stable set or as the probability that the opponent will bluff becomes smaller. In such cases, the uninformed agent would adopt a pooling equilibrium, which does not involve threats. The pooling equilibrium is sometimes characterized by a higher level of weapons than the separating equilibrium. In such cases, arms races that do not lead to war but rather to a situation of deterrence are characterized by higher levels of weapons expenditures than arms races that do lead to war.

The model and its results open up a new approach to the study of outbreak of war. In subsequent work the model will be extended to the case of symmetric uncertainty, uncertainty over technology and resources as well as uncertainty over preferences, private as well as public bads resulting from war, and other similar considerations. By extending the basic model, it will be possible to address such issues as technological change and the qualitative arms race and cooperative solutions to the arms race in the form of arms limitation agreements and other arms control measures.

Appendix

Proposition 1. If $w(i) > 0$ then $\frac{\partial V}{\partial w(i)} > 0$ and thus the resource constraint defined by equation (2) is always binding.

Proof. The i^{th} agent's maximization problem in the first period can be written as

$$\max a[x(i)] + V[w(i), w(j), n(i)]$$

subject to

$$K(i) = x(i) + w(i).$$

$a'[x(i)] > 0$ is sufficient for $\frac{\partial V}{\partial w(i)} > 0$ if $w(i) > 0$.

Theorem 1. For any given initial allocation, $K(i)$, there exists an equilibrium weapons allocation for the reaction functions given by equation (9).

Proof. The conditions of the Berge maximum theorem are satisfied, and $w(i)$ is a single-valued function because $a[.]$ and $V[\ldots]$ are concave. Equations (9) are continuous, and they define a mapping of the compact set $[0,K] \times [0,1]$ into itself. Thus, there exists a fixed point, which is the equilibrium allocation.

Theorem 2. There exists a set of initial allocations that are strictly preferred to the conflict allocation by both agents.

Proof. Since the conditions of the Berge maximum theorem are satisfied, $w(i)$ and $w(j)$ are continuous functions of $K(i)$ and $K(j)$, and $y(i)$ and $y(j)$ are continuous functions of $w(i)$ and $w(j)$. It follows that $y(i)$ and $y(j)$ are continuous functions of $K(i)$ and $K(j)$. Agent maximization therefore defines a continuous mapping from first-period allocations to second-period allocations. This mapping is defined on a compact set and thus has a fixed point. At the fixed point the initial allocation is strictly preferred to the conflict allocation because it avoids the conflict externality without any change in the allocation of the private good. Thus, there exists a neighborhood of the fixed point for which the initial allocation is strictly preferred to the conflict allocation.

Theorem 3. If the transfer $T(i) > 0$, an equilibrium level of weapons exists and the allocation that results from the i^{th} agent adopting a deterrence arms investment strategy and the j^{th} agent adopting a war arms investment strategy is a Nash equilibrium.

Proof. The reaction function defined by equation (14) for the i^{th} agent, together with the reaction function given by (9) for the j^{th} agent, defines a mapping of $[w(i), w(j)]$ from $[0,K] \times [0,K]$ into itself. The mapping (14) is an upper hemicontinuous one by the Berge theorem, so, by the Kakutani fixed point theorem, there exists a fixed point which can be interpreted as the intersection(s) of the two reaction functions. Thus an equilibrium level of weapons exists, however it may not be unique. The second part of the proof is immediate from the construction of the two reaction functions if both agents adopt the appropriate strategies. The deterrence arms investment strategy clearly dominates the war arms investment strategy for the i^{th} agent if $T(i) > 0$. If the i^{th} agent adopts a deterrence arms investment strategy, the optimal strategy for the j^{th} agent is to maximize

$$a[K(j) - w(j)] + a[K(j) + T(i)]$$

subject to

$$V[w(j), w(i)] = a[K(j) + T(i)].$$

This maximization is identical to that defining the war arms investment strategy in the proof of Theorem 1.

Theorem 4. If $T(i) = 0$, and both countries adopt a deterrence arms investment strategy, then the resulting configuration is not a pure Nash equilibrium.

Proof. Suppose both agents adopt a deterrence arms investment strategy. It then is in the interest of each of the agents to reduce its weapons because fewer weapons are needed to deter, given that the other agent has adopted a deterrence arms investment strategy.

Theorem 5. There is a Nash equilibrium in the space of mixed strategies.

Proof. All finite two-person games have an equilibrium in the space of mixed strategies.[21]

Theorem 6.

$$\frac{d\Delta}{dp} = a[K(i)] - \{qV(i) + (1-q)a[K(i) - T(i)]\} > 0.$$

So the relative value of a separating equilibrium increases as the probability that the other agent is of type n_1 increases.

Proof. We can differentiate Δ with respect to p and use the envelope theorem to obtain the stated result.

Theorem 7. There exists an interval of positive measure, $[K_1, K_2]$, *the stable set,* in the interval $[0,K]$ such that the pooling equilibrium will dominate the separating equilibrium for all values of p.

Proof. From Theorem 2, we know that there exists a set of initial allocations that are strictly preferred by both agents if the informed agent is known to be of type n_1 or if it is known to be of type n_2. Consider this set for an agent of type n_2. This distribution of wealth would involve no transfers to either an n_1 or n_2 agent, so the equilibrium is a pooling equilibrium for all p for $K(i)$ in the stable set.

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[21] See Nash (1950). For discussions of the Nash theorem, see Intriligator (1971), and Owen (1982).

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Do New Soviet Leaders Really Make a Difference? Rethinking the "Succession Connection"

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According to an increasingly more widely held view, Leninist regimes tend to pump up mass-oriented policies during succession crises. Yet, this empirically based theory contains significant conceptual and methodological flaws, suggesting the need to reexamine the evidence and rethink the thesis. Using the Soviet evidence, a retest of its hypotheses fails to support this thesis. An alternative, consolidation connection is proposed here which considers the impact of political processes upon a General Secretary's capacity to innovate as well as his incentive structure. Tests of these hypotheses show a significantly higher rate of confirmation than the retest of the original thesis.

According to Bunce (1980, p. 974), new Soviet general secretaries tend "to pump up public consumption in the period immediately following succession" and then "to move toward less popular policies, once the succession crisis has been resolved." This tendency leads to a policy "cycle of mass and then elite-oriented priorities." This cycle is manifest in "an overall pattern of surge and then decline" in mass-oriented policies, including wages and incomes, consumer goods, housing stocks, investments in light industry, agriculture, and housing, and budgetary expenditures for health, education, and welfare.

Bunce (1980, pp. 968, 975) explains that Soviet leaders believe "the masses need to be placated in the period immediately following succession." Specifically, the Soviet leaders seek, on the one hand, to "dampen any mass unrest that may break out during the uncertain times of a succession crisis" and, on the other, to respond to "certain frustrations" that are created by the alternation of mass- and elite-oriented policies. Once the succession crisis passes, the general secretary "placates more traditional constituencies" and his "policies then become less responsive to the public. . . ."

This thesis has received wide attention and acceptance not only in Sovietology (Breslauer, 1982; Brown, 1982; Mills, 1981; Ryavec, 1982), in studies of other Leninist regimes (Esherick & Perry, 1983; Johnson, 1982; Mason, 1983), and in comparative studies of leadership (e.g., Czudnowski, 1982), but also in such disparate areas of study as American judicial behavior and state politics (Caldeira, 1981; Cheek, 1983). The "succession connection" thesis is an important one, for it implies that the study of comparative politics has exaggerated the dissimilarities between liberal democracies and Leninist regimes. It has overlooked essential parallels between succession struggles in states such as the Soviet Union and competitive elections in western democracies. Studies of Leninist systems have overestimated the independence of political leaders and underestimated their responsiveness to the general public.

Successions have long been central to the study of Leninist regimes. The "succession connection" thesis challenges over two decades of research in this field. Most important, it argues that successions in communist systems may not be crises, but essential adaptive mechanisms (cf. Rush, 1968). According to Bunce (1980, p. 966), "new leaders do introduce new policies, and therefore succession can be seen not as a crisis so much as a mechanism of policy innovation" (cf. Bunce, 1976, p. 31; Esherick & Perry, 1983; Mills, 1981). Similarly, it challenges the view that during successions, Leninist regimes such as the Soviet Union are far less capable of dynamic responses to pressing problems (cf. Brown, 1984; Roeder, 1984; Rush, 1968). Instead, it argues, these are periods of major innovation.

More than this, the thesis challenges comparative analyses that find the political processes of Leninist regimes and western democracies to be fundamentally different. First, it argues that in important ways leaders in Leninist regimes are as responsive to the populace as leaders in western democracies. According to Bunce (1980, p. 975), "this, indeed, is the irony of an irregular, elitist model of succession: it makes responsiveness to

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mass demands perhaps as important as it is in a polyarchical system." Second, it argues that at key points the politics and processes of successions in Leninist states parallel those of democratic elections (cf. Johnson, 1982, p. 929). In both, ambitious politicians must build authority, popular constituencies must be propitiated, and a honeymoon gives the new leader relatively free rein to build his authority with the public (cf. Hess, 1977; Mayhew, 1974; Schlesinger, 1966). Third, it argues that the policy consequences of leadership change are much the same in both—it leads to a short-term pumping up of consumer policies (cf. Esherick & Perry, 1982, p. 174; Ryavec, 1982, p. 969). The succession connection, much like the electoral connection, produces the cycle of economic priorities during an administration (cf. Cameron, 1978; Cowart, 1978a,b; Hibbs, 1977; Tufte, 1978).

Critique

Despite the wide acceptance of this thesis, the argument for a succession connection to policy in the Soviet Union is flawed by important conceptual and methodological shortcomings, and these should lead us to reexamine the evidence and rethink the thesis.

There are at least three conceptual problems in the argument. These appear not only in Bunce's original argument, but also in much of the subsequent literature on the succession connection.

The first is to treat the broad Soviet populace as if it were an electorate, like the American public. Bunce (1980, pp. 967, 968) argues that during a succession the general secretary is "building a mandate" or building "a positive base of support for the new regime." Yet, it is unclear why a new general secretary should appeal to the broad populace during a succession crisis rather than to more powerful constituencies such as the military. In Soviet successions, as in American elections, interests receive attention because they can influence the outcome. If there is a Soviet electorate that can appoint and remove leaders in the Soviet Union, it is drawn from the uppermost reaches of Soviet society and disproportionately from the old iron triangle of Party apparatus, heavy industry, and the coercive arms of the State (cf. Bunce, 1976, p. 8). For example, Khrushchev found that it was his alliance with the military, in the person of Marshal Zhukov, and the Party *apparat* that was indispensable in his victory over the Anti-Party Group. It was this alliance, and not the broad populace, that saved Khrushchev in 1957 (Conquest, 1962, p. 318; Pethybridge, 1962, pp. 102-112; Pistrak, 1961, p. 247). It is this proximate electorate that must be satisfied during a succession (Rush, 1968, pp. 84-87).[1] Thus, in a succession period, a Soviet General Secretary cannot defer the demands of this elite electorate and reallocate resources from elite to mass interests.

Insofar as the broader Soviet populace is part of this electorate, in any meaningful sense, it receives consideration from the leadership not by virtue of any immediate power to elect or remove a general secretary. Its power is far more indirect (Friedgut, 1979, p. 289). It can defeat a leader's policies by its malaise, resistance, or sabotage. It can embarrass a leader by widespread unrest (once a leader is firmly enough in control to bear responsibility for policies). Thus, this remote electorate may even prompt the proximate electorate to remove the general secretary (Rush, 1968, p. 88). Yet the record of Soviet politics since 1953 suggests that, unlike workers in Poland, the populace of the Soviet Union has not regularly resorted to its few (and often self-defeating) sanctions in order to intervene in the selection of the general secretary.

Second, Bunce's original argument and subsequent analyses focus almost solely upon the incentive structures of the general secretary as an explanation for policy priorities. They largely overlook the constraints imposed on the general secretary's actions by leadership politics and decision-making processes (cf. Brown, 1982).

It is possible to ignore these constraints only because the "succession connection" thesis contains the simplifying assumption that during the succession there is an unusual unity of perspective among aspiring Soviet leaders. It assumes that all members of the leadership see the structural predominance of the leadership as the most pressing problem they face and thus will defer their particularistic policy concerns over the short run (Breslauer, 1982, p. 286; Bunce, 1980, pp. 968, 974). More than this, however, the argument must posit that all or at least the most powerful members of the leadership agree that the most effective way to secure their predominance collectively is through consumer-oriented policies. Yet, the record of Soviet successions, which have often featured strong disagreements among leaders and, in particular, major disagreements over the allocation of resources between light and heavy industry, does not support this assumption of leadership unity in successions (Conquest, 1962; Ploss, 1965; Rush, 1968).

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[1] As I point out below, this is not limited to the period after a change in leadership. It may come at any time a leader faces strong opposition. (Compare Cameron's (1978, pp. 1246-1247) discussion of the patterns of spending by incumbents facing reelection and an opposition that wins in a democratic polity.)

It is only by making these assumptions that the argument can overlook the constraints imposed by leadership politics upon the capacity of the general secretary to affect policy outputs. The general secretary's incentive to satisfy mass demands may vary with the stages of the succession, but so also will his power to influence policy outputs. In a political system without an institutionalized succession one cannot consider the initial period after the change in leadership as if it were analogous to the honeymoon of a newly elected American president or to the preelection ploys of an incumbent chief executive. A new general secretary is unlike an incumbent American president with constitutionally prescribed powers, surrounded by a new administration of his own design, and with a guaranteed four-year tenure in office. In fact, during the succession the general secretary is more like a candidate, aspiring to a position of power that he does not yet hold (cf. Bunce, 1981, p. 169). A general secretary's powers, to turn around Neustadt's (1960, pp. 34-37) observation about the presidency, rest on his personal power. This personal power is likely to be weakest toward the beginning of a new general secretary's tenure (that is, during the succession period) (Brown, 1982). He is more likely at this point to be constrained in his decision making by his colleagues in a collective leadership. So, it is in a succession period that a general secretary is least able to effect dramatic changes from established patterns of spending when that calls for reallocation of scarce resources at the expense of vested interests near the top. If this is the Soviet equivalent of the president's honeymoon, the general secretary's spouse is likely to be gravely disappointed.

Third, in both Bunce's original argument and in subsequent analyses, the treatment of policy is marred by a certain conceptual imprecision. The thesis fails to distinguish among individual *intentions,* policy *outputs,* and the *outcomes* of policy. The evidence adduced to support the succession connection includes public statements by the general secretary (see Breslauer, 1982, pp. 277-278), decrees of the collective leadership, governmental expenditures, and indicators of economic performance (Bunce, 1980). These fail to consider that the causal nexus between the general secretary's intentions, as expressed in his policy pronouncements, and actual policy outputs is mediated by the politics of Politburo decision making (itself a variable that may be closely correlated with successions) and, thus, may be weak (Schwartz & Keech, 1968; cf. Breslauer, 1982). Nor do they recognize that the causal link between policy outputs and the outcomes of policy is mediated by the uncertainties of bureaucratic administration and environmental constraints and, thus, may also be tenuous (cf. Siegel & Weinberg, 1977).

Moreover, the argument for a succession connection to consumer-oriented policy outputs fails to make even the most rudimentary distinctions among types of consumer policy, such as that between distributive and redistributive policies. A priori one would expect each to show a different connection to successions. Lowi (1964, pp. 689-690, emphasis omitted) argues that each of "these areas of policy or governmental activity . . . tends to develop its own characteristic political structure, political process, elites, and group relations." Distributive policies are characterized by the ease with which they can be disaggregated and dispensed unit by small unit, each unit more or less in isolation from other units. . . ." Alternatively, redistributive policies have the potential for reallocating scarce values among key interests and thus are more likely to produce intersectoral disputes (Siegel & Weinberg, 1977; Smith, 1975).

For example, in a command economy policy decisions to raise wages may have a different connection to successions than policy decisions to appropriate additional budgetary or investment funds to a program. The former are distributive, but not necessarily redistributive in the short term; the latter tend to be redistributive even in the short run. Raising wages can be a popular policy with the mass of the population. Yet it does the least to affect the allocation of scarce resources in society and hence affects elite interests the least. In the absence of a reallocation of real resources, such as investment funds, a boost in wages can simply lead to hidden inflation or galloping savings (Nove, 1982, pp. 381-382).

This conceptual imprecision is further reflected in the description of at least two different and contradictory patterns of policy that result from the "succession connection." On the one hand, in the initial statement of the succession connection thesis, Bunce (1980, p. 966) describes not a cycle but a "ratchet" phenomenon. "Socialist leaders seem to rearrange priorities in their first year in office; however, once these innovations are introduced, the new priorities tend to freeze—with incremental adjustments—until the next succession." That is, succession leads to a redirection of policy toward mass priorities that persists throughout a leader's tenure. On the other hand, throughout most of the argument, Bunce describes a true policy cycle of "surge and then decline." Each of these two views describes a very different pattern of investment, expenditure, and production. If the "ratchet" effect is the appropriate description, mass-oriented policies would claim an ever-growing portion of Soviet resources over time. Successions would, in fact, be important adaptive mechanisms, setting new directions in policy. But if the cycle is the appropriate metaphor, then successions do not lead to a redirection of policy over the longer term, but only to a brief and transient expansion of mass policies. Thus, successions are only stages of a cycle, in which priorities always return to their well-established patterns. (This second view is, in fact, the one Bunce most consistently argues, and it is this relationship that she attempts to test. The discussion below will, thus, address this.)

In addition to these conceptual problems, there are at least three methodological flaws in the original empirical evidence for a succession connection to consumer-oriented policy. These are important, because subsequent studies of the succession connection have commonly posited that Bunce's thesis has been confirmed by the Soviet evidence.[2] All of these flaws should lead us to question whether Bunce's empirical evidence in support of the succession connection thesis might not, in fact, be spurious.

First, Bunce's tests fail to control for the effect of uneven demographic changes in this period. Most important, with a long-term or secular decline in population growth rates, the average annual population growth rate was higher in each succession period than in the post-succession period that followed. Indeed, the rate was 15.3% higher in the former periods, and fully 22.9% higher in the Brezhnev succession years, than the post-succession years (Russia, 1971, p. 7; 1976, p. 7). Even in the absence of a succession connection, if Soviet investment in and production of consumer goods, investment in and construction of housing, and budgetary allocations to health, education, and welfare were made on unchanging formulaic grounds (for example, reflecting demographically determined need), the succession years would show proportionately higher spending and higher growth rates than the post-succession years that followed. Is the pattern that Bunce finds a consequence of politics or demographics?

Second, Bunce's comparisons of investment and spending levels in different periods typically do not control for any long-term or secular trends in the policies themselves. For example, there has been a steady downward trend in the proportion of capital investment allocated to housing. This proportion has steadily declined with each new five-year plan beginning with the sixth (1956-1960), falling an average of 3.3% per year from 1958 to 1975 (Russia, 1963, p. 434; 1968, p. 619; 1971, p. 483; 1976, p. 507). Thus, even in the absence of any surge-and-decline cycle, there would be higher levels of investment in housing during the succession periods than during the post-succession periods that followed. Is the pattern that Bunce finds a result of politics or long-term trends?

Third, the use of proportionate measures of budgetary and investment expenditures may not always indicate changing priorities of the leadership. The totals in the denominators of these measures—that is, total state budget and total capital investment—have not been consistent measures over time. For example, at various times the Soviets have included expenditures in the budget that were previously excluded and vice versa. Thus, the rise in the proportion of the Soviet budget given to health, education, and welfare in the 1965-1968 period in particular may not reflect a shift of priorities. It may reflect the tendency, accentuated by the industrial reform of 1965, "away from budget financing of the national economy" (Hutchings, 1982, p. 173); that is, in the second half of the 1960s, the budget total included a declining proportion of total spending on some policies that Bunce labels elite priorities. As a consequence, the apparent proportionate effort for sociocultural measures rose in the mid-sixties. When the practice was reversed toward the end of the decade, the apparent proportionate effort for sociocultural measures again fell. Is the surge-and-decline a consequence of the succession connection or changing accounting procedures?

Outputs and Outcomes: A Second Look at the Evidence

These problems in the argument and methodology supporting the succession connection suggest the necessity of reexamining the evidence. Because the argument for this succession connection runs counter to a considerable body of current sovietological "wisdom," the strength of this argument rests heavily upon its empirical tenability. In particular, the strength of this argument depends on its accuracy as a description of Soviet policies and performance in the post-Stalin period.[3]

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[2] Only recently has the applicability of this thesis to non-Leninist states come under serious criticism (cf. Brunk & Minehart, 1984). No systematic reexamination of the findings in any of the Leninist regimes has been attempted, so even critics of the original succession connection thesis often posit that the results support the thesis (e.g., Brown, 1982).

[3] Although Bunce's article includes some brief discussion of the Stalin regime and of three East European socialist states, "the main concern" of the article is "the Soviet Union in the post-Stalin period" (Bunce, 1980, p. 968, n. 2). Breslauer (1982) focuses solely upon the period since 1953. A priori, it seems inappropriate to compare the pre-Stalin with the post-Stalin years. For example, the Soviet leadership did not enjoy the same

The most systematic tests of the succession connection to policy outputs and outcomes in a Leninist regime have been undertaken by Bunce. These tests use nine indicators of mass-oriented policies. Five of these might properly be said to reflect policy outputs, including the measures of budgetary expenditures on sociocultural measures, welfare expenditures, investment in light industry, housing investment, and wages. These are the most controllable and hence responsive to leadership decisions. Four others, including national income, light industry production, housing construction, and pension recipients, are performance indicators or policy outcomes and are less responsive to leadership decisions.

All but one of these nine indicators show a stable long-term growth rate over the two decades from 1954 to 1974.[4] A simple trend line fitted to the natural logarithms of each indicator accounts for more than 99% of the variance of budgetary expenditures on sociocultural measures, of national income, and of light industrial production, and accounts for more than 98% of the variance of wages, 96% of welfare expenditures and pension recipients, 95% of light industry investment, and 89% of investment in housing. Only housing construction does not show this, with a trend line accounting for only 30% of the variance.

The question posed by the argument for a succession connection to mass-oriented policies is whether in succession periods the Soviet leadership spends, invests, or produces *above the trend.* If they do, one would expect to find significantly higher levels of expenditure, investment, and production for each of the nine indicators in the two succession periods (1954-1957 and 1965-1968).

To test this idea, each of the five indicators of policy outputs is included as a dependent variable in a separate regression equation controlling for secular trend and population.[5] The test of the succession connection is the significance of two dichotomous variables, one for each of the two succession periods between 1954 and 1974. If there is a surge in mass-oriented expenditures in the succession periods, the parameter estimates should be positive and significant at the .05 level, which would indicate a statistically significant, higher level of mass-oriented expenditure in these years.

In the five equations, there are nine parameter estimates for the succession connection. (See Table 1).[6] Of these nine estimates only one is, in fact, both positive and significant at the .05 level (one-tailed test); that is, only one of nine confirms the original succession connection thesis. Of the remaining eight, only three are even positive, whereas five are negative. Interestingly, the one confirming case—average wage during the Stalin-Khrushchev succession—is associated with the sole distributive, as opposed to redistributive, policy.

If the four indicators of economic performance or policy outcomes are regressed in a similar manner on the dichotomous and "control" variables, there are six parameter estimates for the succession periods. (See Table 2.) Of these six, none is positive and significant at the .05 level. In fact, only one is even positive, and of the five negative estimates, two are significant at the .05 level. In short, the pattern of results suggests that Soviet economic performance in a succession does not favor the interest of the mass, as defined by the original argument for a succession connection, but may actually discriminate against it.

In sum, the evidence does not support the succession connection thesis. Of the 15 tests in these equations, only one confirms this argument at the .05 confidence interval, and two-thirds have signs diametrically opposite to that predicted by this argument. None of the tests for the redistributive policies and only one of the tests for the dis-

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degree of control over the economy before Stalin began the five-year plans. Similarly, successions in the East European socialist states are quite different from those in the Soviet Union. Most significantly, in East European, unlike Soviet, successions there is an independent final arbiter with the power (and often the authority) to select definitively a new leader and end the crisis—the Soviet Union.

[4] These tests are limited to the period from 1954-1974 for two reasons. First, these years were selected in order to insure that the two succession cycles are comparable. I compare the succession period, as demarcated by Bunce, and the first six years of post-succession in the Khrushchev and Brezhnev regimes, which avoids any difficulties that emerge if late post-succession periods (of which there was none under Khrushchev) are intrinsically different from early post-succession periods. Second, the overall slowing or downturn of the Soviet economy in the second half of the seventies introduces an extraneous variable that makes it difficult to discern the independent effect of any succession connection. For these reasons, these tests exclude the late Brezhnev years of slowing economic growth.

[5] Each indicator was regressed on alternative measures of relevant demographic changes, including the natural logarithms of the population, the net increase in population, and the net increase of the urban population. The demographic control variable was chosen by selecting the variable that offered the best fit as measured by the coefficient of determination of the equation and the significance of the demographic control parameter.

[6] The annual data for several of the indicators used in this and later tables are incomplete for the 1950s, so that there is no dummy variable for the Stalin-Khrushchev succession in several equations.

Table 1. Regression Results: Soviet Budgetary Expenditures, Investments, and Wages, 1954-1974

	Parameter Estimates	t	R^2	F
Equation (1). Sociocultural Expenditures				
Succession 1	-0.044	-1.09		
Succession 2	0.016	0.67	0.996	
lnPopulation	1.207	1.03		
Time	0.063	4.34		922.85
(Intercept)	-4.111	-0.46		
Equation (2). Welfare Expenditures				
Succession 1	-0.159	-1.75		
Succession 2	-0.044	-0.82	0.982	
lnPopulation	0.536	0.20		
Time	0.072	2.19		198.65
(Intercept)	0.049	0.00		
Equation (3). Investment in Light Industry				
Succession 1	-0.209	-1.64		
Succession 2	0.110	1.48	0.965	
lnPopulation	-5.095	-1.36		
Time	0.147	3.17		96.77
(Intercept)	45.603	1.60		
Equation (4). Investment in Housing				
Succession 2	0.012	0.12		
lnNet Population Increase	-0.446	-1.42	0.934	
Time	0.040	2.77		
(Intercept)	10.240	8.32		52.04
Equation (5). Wages				
Succession 1	0.060[a]	3.32		
Succession 2	-0.012	-1.04	0.992	
Time	0.040	39.03		
(Intercept)	6.458	485.60		647.54

[a] Parameter estimate associated with the dichotomous variables that is significant at the .05 level (one-tailed test).

tributive policy supports the thesis. Does this mean that new Soviet leaders really do not make a difference? Are there no structural limitations associated with the succession that affect policies in ways that disrupt longterm trends and transcend the idiosyncracies of the general secretary? In fact, the evidence does suggest that there are "temporal" similarities in policy orientation that are closely tied to the succession process.

An Alternative Succession Connection

Elsewhere, in a discussion of Soviet foreign policy (Roeder, 1984) I argue that the attributes of Soviet policy are in part a consequence of the prevailing policymaking mode in the Soviet leadership (called "regimes").[7] These regimes are in turn the result of shifts within the leadership along two dimensions—the level of competition for power within the leadership and the extent of consolidation of decision-making authority in the hands of the general secretary.[8] In short, changes in the distribution of power lead to changes in

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[7] The assumption of a causal link among power, process or regime, and policy syndromes is not original with this study (cf. Schilling, Hammond, & Snyder, 1962, p. 24). It was studied by Brzezinski and Huntington (1965, pp. 191-232) in their classic comparison of American and Soviet polities. Allison (1971) has made a forceful argument for the link between proccess and policy. Hermann (1978) has developed an explicit comparative scheme. Insofar as these studies bear on Soviet politics and policy, however, they have treated power, process, and policy as static, rather than changing over time.

[8] These distinctions are similar—although not identical—to Dahl's distinction among regimes on the dimension of contestation and participation (Dahl, 1971, pp. 4-8). In the present case, the variation among regimes is much narrower, because it considers the level of contestation and participation within a closed hegemonic leadership.

Table 2. Regression Results: Soviet Economic Performance, 1954-1974

	Parameter Estimates	t	R^2	F
Equation (1). National Income				
Succession 1	-0.046[a]	-2.96		
Succession 2	0.009	0.97	0.999	
lnPopulation	-0.844	-1.88		
Time	0.069	12.35		3361.7
(Intercept)	12.329	3.61		
Equation (2). Consumer Goods Production				
Succession 1	-0.023	-1.57		
Succession 2	-0.017	-1.91	0.999	
lnPopulation	-1.190	-2.74		
Time	0.072	13.46		3342.1
(Intercept)	14.726	4.47		
Equation (3). Housing Production				
Succession 2	-0.004	-0.12		
lnNet urban population increase	0.151	1.24	0.416	
Time	0.007	2.55		
(Intercept)	6.328	14.38		2.62
Equation (4). Pension Recipients				
Succession 2	-0.005	-0.13		
lnRetirement-aged population	1.961	1.19	0.971	
Time	-0.001	-0.02		
(Intercept)	-23.656	-0.98	132.46	

[a] Parameter estimate associated with the dichotomous variables that is significant at the .05 level (one-tailed test).

the mode of decision making, and these lead to changes in the attributes of policy.

Soviet Regimes

Based on differences in the level of power consolidation and competition within the Party leadership, it is possible to identify six Soviet regimes in the two decades from 1954 to 1974. For example, *pluralistic* policymaking, characterized by high levels of elite competition and wide dispersal of policymaking authority, prevailed during the Stalin-Khrushchev succession until in June, 1957, Khrushchev eliminated his major opponents in the Anti-Party Group (Rush, 1968, pp. 42-71).

After Khrushchev assumed power, beginning with the second half of 1957, policymaking went through two phases—directive and primatial. *Directive* policymaking, characterized by very limited competition for power and the consolidation of much decision-making authority in Khrushchev's hands, prevailed until the emergence of an embryonic opposition in early 1960 (Slusser, 1967, p. 194). *Primatial* policymaking followed until Khrushchev's removal in October, 1964. This regime was characterized by Khrushchev's continued control over policy, but growing opposition within the leadership. This opposition apparently came from a coalition that initially centered on Suslov and Kozlov, but grew to include a majority of the Politburo (Brown, 1971, p. 125; Daniels, 1971, pp. 21-22; Linden, 1966, pp. 90-201; Slusser, 1967, pp. 200-260).

The Brezhnev oligarchy that came to power in October, 1964, brought with it a new style of policymaking that in the years up to 1974 went through three phases—competitive oligarchic, stable oligarchic, and cartelistic. *Competitive oligarchic* policymaking, which is characterized by wider dispersal of authority among the oligarchs and competition for power among the conspirators who ousted Khrushchev, prevailed during the succession period of the early Brezhnev years. This succession period was distinguished from the Stalin-Khrushchev succession, however, in that the levels of competition within the leadership were lower and decision-making authority was less widely distributed than under the triumvirate of Malenkov, Molotov, and Khrushchev. This mode of decision making continued until the summer of 1967, by which time the "conservative center" around Brezhnev had apparently succeeded in containing the power or challenges of Podgornyi and Shelepin (Adomeit, 1981, pp. 72-73; Brown, 1978, pp. 219-220, 251-252; Dallin, 1973, pp. 49-50; Hammer, 1974, p. 321). *Stable oligar-*

chic policymaking, characterized by the consolidation of some decision-making authority in the hands of Brezhnev and low levels of elite competition, prevailed after this until 1971. By that year, competition had again increased as disagreements over Brezhnev's (failed) policies rose. It was highlighted by the slow easing of Shelest and Voronov from the leadership (Hammer, 1974, p. 323; Osborn, 1974, p. 201). The outcome was a slight decline in the consolidation of policy-making authority in Brezhnev's hands (Breslauer, 1982, pp. 200-219). Policymaking had become *cartelistic.*

Rankings of these six regimes according to the level of elite competition can be estimated by the turnover (owing to reasons other than death) among members of the Politburo and Secretariat (cf. Hough, 1976, p. 3). This turnover can be measured by the probability that an individual member of the Politburo or Secretariat will survive in office for 12 months. These probabilities for each period are shown in Table 3 and for each year are graphed in Figure 1. Of course removal from office is often only the final event in a struggle for power. To estimate a coefficient of competition (*CC*), it is appropriate to discount each annual survival probability by the square of the probability of being ousted in the next year, the cube of the probability in the year after that, and so on.[9] These annual coefficients can be graphed to show a fever chart of Soviet elite competition (see Figure 1). On the basis of the evidence summarized in Figure 1 and Table 3, it is possible to estimate the rankings of the six regimes according to the level of elite competition in each. From highest to lowest this rank order would be—1) pluralistic (Stalin-Khrushchev succession), 2) primatial (late Khrushchev era), 3) competitive oligarchic (Khrushchev-Brezhnev succession), 4) cartelistic ("late" Brezhnev era), 5) directive (Khrushchev's ascendancy), and 6) stable oligarchic (Brezhnev's ascendancy).

The rankings of regimes according to the consolidation of authority over policy must rely upon more descriptive evidence. Western analyses of the Khrushchev and Brezhnev years suggest four observations that are relevant to this estimation. First, Khrushchev's control over policy between 1957 and 1964 was greater than Brezhnev's between 1965 and 1974 (Bialer, 1976, p. 31; Breslauer, 1982, p. 17; Brown, 1978, p. 234; Hammer, 1974, p. 319). Second, Khrushchev's rule was increasingly challenged toward the end of his tenure (Breslauer, 1982, pp. 115-133; Daniels, 1971, p. 21; Linden, 1966; Slusser, 1967). Third, and in contrast, Brezhnev's control tended to grow with time, even though he suffered a slight setback in the early seventies (Breslauer, 1978, pp. 15, 19; 1982, pp. 200-219; Brown, 1978, p. 254). And fourth, the least consolidation between 1954 and 1974 occurred during the collective leadership of the Stalin-Khrushchev succession. On the basis of this evidence, it is possible to estimate the ranking of the regimes according to their level of consolidation. From highest to lowest this rank-order would be—1) directive (Khrushchev's ascendancy), 2) primatial (late Khrushchev era), 3) stable oligarchic (Brezhnev's ascendancy), 4) cartelistic ("late" Brezhnev era), 5) competitive oligarchic (Khrushchev-Brezhnev succession), and 6) pluralistic (Stalin-Khrushchev succession). (These rankings of the regimes are summarized in columns 5 and 6 of Table 3.)

Attributes of Policy

A surge in mass-oriented policy *outputs* will depend not simply upon the incentive structure of the general secretary, as the original argument for the succession connection seems to suggest. A surge in these policy outputs will also depend upon the capacity of the secretary to innovate or take bold steps that deviate from the norm. These two attributes of a regime—which I designate *incentive* and *innovativeness*—will, in turn, depend upon the consolidation of authority within the Soviet leadership and the level of competition among these leaders.

Innovativeness. On one hand, the capacity for innovation in the Soviet Union is closely tied to the consolidation-participation dimension (cf. Rush, 1974, pp. 14-19). Specifically,

HYPOTHESIS 1. The capacity of a general secretary to innovate varies directly with his consolidation of decision-making authority.

With broad participation (or low consolidation), the Soviet regime approximates committee decision making, and Soviet policy comes to reflect the strengths and weaknesses of that mode. Huntington (1961, p. 167) catalogs the criticism of such decision making in American politics: Committee decision making leads to policy that lacks "unity, coherence, and a sense of direction"; it leads to compromise, avoiding choices among clear-cut alternatives; the process is slow and

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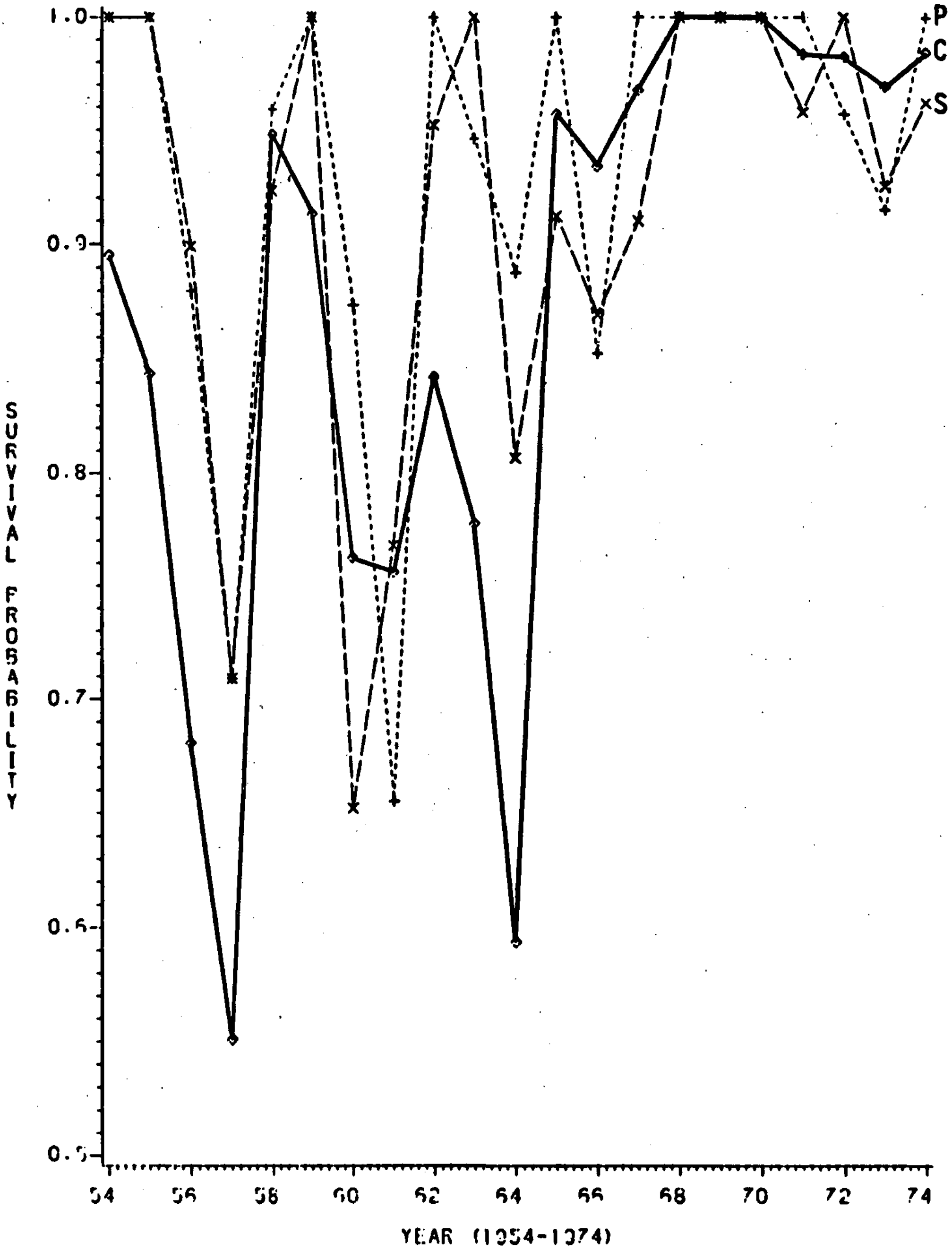
[9] Specifically, the survival probability is calculated according to the formula $p = [1-(d/m)]^{12}$, where d is the number of officials who lost their positions (except due to death), and m is the number of man-months served by all office holders. The coefficient of competition for each year (t) is calculated according to the formula,

$$CC = [1 - \sum_{t=1} (d/m)^t]^{12}.$$

Figure 1. Soviet Elite Competition



C: Coefficient of competition[a]
P: Politburo survival probability
S: Secretariat survival probability

[a]This coefficient of competition is the average of the coefficient for the Politburo and Secretariat and the coefficient for the First or General Secretary.

Table 3. Comparison of Soviet Regimes, 1954-1974

Regime	Dates	Survival Probability[a]		Competition[b] (Rank)	Consolidation Innovation[c] (Rank)	Incentive[d] (Rank)		Policy Outputs[e] (Rank)	
		Politburo	Secretariat			Mass	Elite	Mass	Elite
Pluralistic	3/53- 6/57	.853	.613	1	6	6	1	6	3
Directive	7/57- 4/60	.971	.966	5	1	2	5	1	2
Primatial	5/60-10/64	.845	.804	2	2	5	2	3	1
Competitive oligarchic	11/64- 9/67	.928	.791	3	5	4	3	5	4
Stable oligarchic	10/67-12/70	1.000	.976	6	3	1	6	2	6
Cartelistic	1/71-12/74	.949	.969	4	4	3	4	4	5

[a] Probability of a member in each body surviving in office for one year.

[b] Rank-order of regimes, based on the average of columns 3 and 4.

[c] Rank-order of regimes according to their level of consolidation of authority and innovativeness, based on the descriptive evidence presented in the text and Hypothesis 1.

[d] Rank order of regimes according to the incentive of the general secretary to satisfy either mass or elite demands, based on Hypotheses 2 and 3. Mass incentive is the inverse rank order of column 5; elite incentive is identical to column 5.

[e] Rank order of regimes according to the level of mass or elite orientation in policy outputs. Mass orientation is the rank order of the products of columns 5 and 7; elite orientation is the rank order of the products of columns 5 and 8.

prone to delays; it fails to yield new ideas and approaches to new problems; and it tends "to magnify the obstacles and difficulties of any proposed course of action."

Similar observations have been made about Soviet policy in the Brezhnev period, when decision-making authority was more widely dispersed than in the Khrushchev years that preceded it. Hough (1973, p. 163) argues that after Khrushchev's removal, Soviet policy was characterized by "a set of political outcomes that (to the extent that accommodation of the demands of some groups requires restrictions upon other groups) embodies only incremental change and that are accommodating to the disadvantaged group." Rigby (1970, p. 190) argues that "there is evidence of hesitant and contradictory decision-making and simple avoidance of decision in several major policy areas." And Bialer (1976, p. 32) argues that this regime "is ill-suited to accommodate innovative ambitions which stray much beyond the existing consensus. . . ." Bunce herself in an earlier article (1978, p. 916) makes such a connection between consolidation and innovativeness, arguing that "while there may not be as many actors involved in decision-making as in the West, it is quite plausible to suggest that the numbers are often sufficient to produce a deadlock and thus stifle major innovations."

Incentive. On the other hand, the incentive of the general secretary to satisfy either elite or mass "demands" is closely tied to the level of competition within the Soviet leadership. Specifically,

HYPOTHESIS 2. The general secretary's incentive to satisfy elite demands rises with the level of competition within the leadership.

When facced with opposition, the general secretary seeks to strengthen his position by appealing to the most powerful constituencies for support. With the marked power disparity between elite and mass, and the virtual absence of any mass power to appoint or remove a leader in the Soviet Union, the "electorate" to whom the power aspirant must appeal is the elite. Thus, for example, the chief competitors in the Stalin-Khrushchev succession appealed to competing elites. "Malenkov's strategy from the outset was largely based on the state bureaucracy," particularly the economic bureaucracy. Initially, "Khrushchev limited his appeal for support chiefly to the Party apparatus, while criticizing the economic bureaucracy, and for the most part neglected popular demands for a better material life" (Rush, 1968, pp. 60, 66). It is true, as the original succession connection thesis argues, that in these competitive periods the structural predominance of the leadership is most vulnerable. Yet it is the "tragedy of the collective," so to speak, that no one aspirant has an incentive to attend systematically to this shared interest when it conflicts with his particular interests and with those of his coalition.

The level of competition within the leadership is related in an opposite fashion to the incentive for the general secretary to attempt to satisfy mass demands. Specifically,

HYPOTHESIS 3. The general secretary's incentive to satisfy mass demands is inversely related to the level of competition within the leadership.

It is when the leader is not challenged in his position that he can attend to the collective needs of the leadership, including its continued predominance within Soviet society. It is at this point that there is greatest identity between his particular interests and the collective interests of the leadership. It is at this point that the leader can force sacrifices upon elite interests in order to spend on those mass-oriented policies that can prevent the mass unrest mentioned by Bunce. But more than this, the general secretary may find a strong attraction to such policies at this time. Not only does the general secretary find it easier to respond to these mass priorities; he may actually find such a policy a promising vehicle for further consolidating his position. A general secretary who seeks to rid himself of the limitations on his personal power imposed by his erstwhile coalition partners may use mass mobilizational policies as one means. A leader may seek to build a "personal" relationship with the mass in order to circumvent the intermediate elites.[10]

These hypotheses are, of course, very different —diametrically opposite in some ways—from those implied in the original succession connection thesis. Although the original thesis implies high levels of innovation during the dispersed authority of the succession, Hypothesis 1 predicts "normal" spending. Although the original thesis implies a high incentive for mass-oriented policies in periods of competition, Hypotheses 2 and 3 predict a low incentive to satisfy mass needs and a high incentive to satisfy elite demands.

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[10] The theory of the direct alliance between leader and mass is, of course, most fully developed in the literature on totalitarianism (e.g., Arendt, 1951, pp. 24-37; Inkeles, 1954, pp. 87-107). Without judging the applicability of the theory of totalitarianism to post-Stalinist Soviet society, it is possible to draw a parallel between this insight and the attempts by Khrushchev to build up his personal power. What is sometimes called Khrushchev's "populism" after 1957, with its surge in domestic mass-oriented spending and its emphasis on *privlechenie* (drawing in), the expansion of rights, and the rotation of cadres, may be interpreted as an attempt by the leader to build a personal relationship with the mass that circumvented the intermediate levels of the party and state (cf. Breslauer, 1976, p. 1982).

Policy Outputs. Periods in which there should be surges in mass- or elite-oriented allocations, expenditures, and performance can be predicted from these three hypotheses and the previous rankings of regimes along the two dimensions of competition and consolidation. Treating orientation in policy outputs as the product of a multiplicative relationship between innovativeness and incentive, the predicted rankings of elite-orientation and of mass-orientation in policy outputs shown in columns 9 and 10 of Table 3 result.[11] These predict that the highest levels of elite-orientation in policy outputs should have occurred in the primatial period (late Khrushchev era) and the highest levels of mass-orientation in policy outputs should have occurred in the periods of directive and stable oligarchic policy-making (Khrushchev's ascendancy and Brezhnev's ascendancy, respectively). In short, mass orientation is highest after the succession is resolved and the newly ascendant leader is consolidating his position—a "consolidation connection."

Second Tests

To these these predictions, the two dichotomous variables for the succession periods in the nine equations for economic policy outputs and economic performance are replaced by three dichotomous variables—two for the periods of predicted high mass orientation in policy outputs and one for the period of predicted high elite orientation in policy outputs. (As a shorthand, the first two are labelled periods of consolidating leadership in the tables and the last is labelled a period of challenged leadership.) This method leads to 27 tests of the predicted relationships. To confirm the predictions, the parameter estimates for the periods of consolidating leadership should be positive and significant at the .05 level, whereas the parameter estimates for the period of challenged leadership should be negative and significant at the .05 level.

Four of the five equations for Soviet economic *policies* in Table 4 show a better fit to the data (as measured by the coefficient of determination adjusted for degrees of freedom) than do the equations in Table 1.[12] (The exception is the equation for wages—again, the sole distributive policy.) Of the 15 parameter estimates for the dichotomous variables, 12 estimates are consistent with the predictions in Table 3. Nine of these are significant at the .05 level (one-tailed test). Of the three estimates that are inconsistent with the predictions, only one (welfare expenditures in the Khrushchev consolidating period) is also significant.

The four equations for Soviet economic *performance* in Table 5 all fit the data as well as or better than the equations in Table 2.[13] Of the 12 parameter estimates, nine are consistent with the predictions in Table 3. Six of these are significant at the .05 level (one-tailed test). Of the three parameter estimates that are inconsistent with predictions, none is significant at the .05 level.

In short, not only do all but one of these equations better fit the data than the equations testing the original succession connection, the parameter estimates "confirm" the predictions more consistently than do the parameter estimates in the former equations. Although only one of the 15 tests of the original succession connection permits rejection of the null hypothesis at the .05 level, 15 of the 27 tests in these latter equations permit rejection of the null hypothesis at this confidence interval.

The hypotheses put forward also predict that there should be surges in elite-oriented policies during some periods—specifically, in the period of challenged leadership (1962-1964). This contradicts Bunce's (1980, p. 970) argument that although "mass-oriented policies show a cycle, heavy industry does not" (cf. Breslauer, 1982, p. 277). Two indicators of elite-oriented policy outputs and outcomes are defense expenditures (*D*) (Shishko, 1977) and heavy industry production (*A*) (Russia, 1965, 1975). Regressing these on the "control" variable for trend (*T*) and the dichotomous variable for period of challenged leadership (*C*) yields the following equations and parameter estimates:

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[11] There are, of course, two important shortcomings in this analysis of rankings: First, it treats the distance between each step in the rankings as equal, and second, it treats the slopes of each hypothesized relationship as equal in absolute magnitude. As a consequence of these shortcomings, I can only identify the periods of greatest elite or mass orientation in policy; I cannot predict how much greater the orientation will be in these periods than in the lower-ranked periods.

[12] The dichotomous variables assume a value of one for the years 1957-1960 (consolidating leadership 1), 1962-1964 (challenged leadership), and 1968-1970 (consolidating leadership 2).

[13] In these tests using indicators of economic performance, the dichotomous variables assume a value of one for the years 1958-1961, 1963-1965, and 1969-1971, respectively; that is, the periods are lagged one year in these tests of performance, because the consequences of policy decisions may not show up in performance until the following year.

$$\ln D = 5.23 + 0.05T + 0.10C \qquad R^2 = 0.970$$
$$t = (177.2)\ (22.8)\ \ (3.0) \qquad F = 260.8$$

$$\ln A = 6.13 + 0.08T + 0.03C \qquad R^2 = 0.998$$
$$t = (566.0)\ (89.2)\ \ (2.0) \qquad F = 3980.0$$

Both of the parameter estimates for the period of challenged leadership are positive and significant at the .05 level. In short, as the leader faces growing competition, but has the consolidated power to innovate, he tends to pump up elite-oriented policies.

Insofar as this pattern describes a policy cycle with a succession connection, it is very different from the cycle described by the original succession connection thesis. The surge in mass-oriented policy comes not in the succession period, but in the early post-succession period when the newly ascendant leader is consolidating his position. The surge under Khrushchev was in the late fifties, after his victory over the Anti-Party Group; the surge under Brezhnev came in the late sixties, after his success in containing the power of Podgorny and the *Shelepintsy.* Figure 2 shows the deviation of Soviet economic policies and performance from the trend (defined by secular trend and population growth), the surge in mass-oriented policy in the consolidating-leadership periods, and the sharp decline in the challenged-

Table 4. Regression Results: Soviet Economic Policies, 1954-1974 (An Alternative Model)

	Parameter Estimates	t	R^2	F
Equation (1). Sociocultural Expenditures				
Consolidating 1	-.007	-0.39		
Consolidating 2	0.024[a]	3.25	1.000	
Challenged leadership	-0.043[a]	-4.89		
lnPopulation	1.466	2.65		4359.2
Time	0.058	9.35		
(Intercept)	-6.048	-1.44		
Equation (2). Welfare Expenditures				
Consolidating 1	-0.067[a]	-2.38		
Consolidating 2	0.037[a]	2.94	0.999	
Challenged leadership	-0.012	-0.82		
lnPopulation	-4.353	-4.69		1542.1
Time	0.126	12.21		
(Intercept)	37.237	5.28		
Equation (3). Investment in Light Industry				
Consolidating 1	0.231[a]	2.28		
Consolidating 2	0.141[a]	3.13	0.986	
Challenged leadership	-0.115[a]	-2.21		
lnPopulation	5.542	1.67		151.79
Time	0.023	0.62		
(Intercept)	-35.261	-1.40		
Equation (4). Investment in Housing				
Consolidating 1	0.100[a]	2.25		
Consolidating 2	0.029	0.89	0.984	
Challenged leadership	-0.170[a]	-4.73		
lnNet Population Increase	1.564	0.15		137.14
Time	0.054	7.50		
(Intercept)	8.588	39.49		
Equation (5). Wages				
Consolidating 1	-0.007	-0.46		
Consolidating 2	0.017	1.09	0.992	
Challenged leadership	-0.047[a]	-3.05		
Time	0.036	32.51		431.90
(Intercept)	6.510	430.23		

[a] Parameter estimate associated with the dichotomous variables that is significant at the .05 level (one-tailed test).

Table 5. Regression Results: Soviet Economic Performance, 1954-1974 (An Alternative Model)

	Parameter Estimates	*t*	R^2	*F*
Equation (1). National Income				
Consolidating 1	0.031	1.58		
Consolidating 2	0.011	1.34	0.999	
Challenged leadership	-0.025[a]	-2.58		
lnPopulation	1.797	2.04		1883.3
Time	0.047	5.01		
(Intercept)	-8.016	-1.19		
Equation (2). Light Industry Production				
Consolidating 1	-0.021	-1.22		
Consolidating 2	0.018[a]	2.44	0.999	
Challenged leadership	-0.015	-1.70		
lnPopulation	-2.933	-3.77		1812.6
Time	0.090	10.79		
(Intercept)	28.013	4.73		
Equation (3). Housing Production				
Consolidating 1	0.102[a]	2.68		
Consolidating 2	-0.012	-0.61	0.848	
Challenged leadership	-0.050[a]	-2.49		
lnNet Urban Population Increase	-0.088	-0.90		10.055
Time	0.009	3.64		
(Intercept)	7.129	21.81		
Equation (4). Pension Recipients				
Consolidating 1	-0.013	-0.30		
Consolidating 2	0.052[a]	2.70	0.993	
Challenged leadership	-0.099[a]	-4.50		
lnRetirement-aged population	3.849	2.86		271.94
Time	-0.060	-1.60		
(Intercept)	-51.224	-2.59		

[a] Parameter estimate associated with dichotomous variables that is significant at the .05 level (one-tailed test).

leadership period. The succession periods, conversely, are close to the "norm" of the trend line.

Figure 2 also suggests a misleading conclusion that might result from concentrating on annual percentage growth rates in mass-oriented policies and performance (as Bunce does in some tests). High percentage growth over the previous year can indicate either a surge in mass-oriented policy above the norm or simply a return to the norm after a year of unusually low mass-orientation in policy. The high percentage growth during the Khrushchev-Brezhnev succession, that is, in 1965 and 1966, for example, seems to represent the second—a return to the norm. Rather than continuing the rapid growth once mass-oriented policies had recovered their "normal" levels, the Soviet leadership in this succession period continued spending at normal levels in 1967. The surge above the norm came only later—in the period of Brezhnev's consolidating leadership.

Intentions and Deeds

Breslauer (1982) builds his case not on expenditures, but on the speeches of Soviet leaders. Also, at the end of her discussion of Soviet expenditures, Bunce (1980, p. 968) argues that "it is . . . important to look not just at outputs, but also at intentions. There may be failures in implementation, and leaders may use rhetoric rather than deeds to get across their concerns." After examining the speeches of Khrushchev and Brezhnev as well as a tabulation of Central Committee plenums by topic, Bunce (1980, p. 972) claims to find the same "cycle of shifting concern with mass demands . . . both in elite rhetoric and in concrete policy priorities" (p. 972). Breslauer (1982, pp. 277-278) supports this view, but would amend the two-stage model with a three-stage model, arguing that innovation continues into the period of ascendancy.

Figure 2. Percentage Deviation from Secular Trend in Soviet Economic Policies and Performance, 1954-1974 (Index = 100)

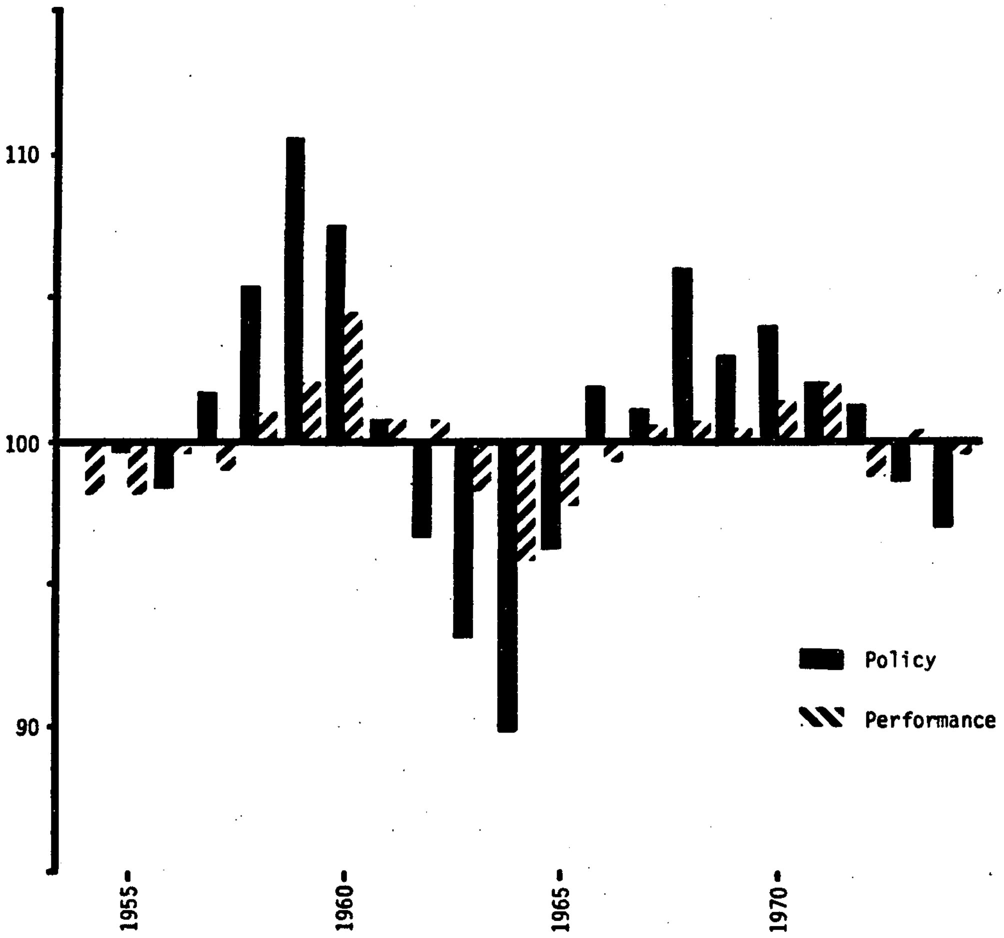

Figure 2 shows the average percentage deviation of actual expenditures and production from the predicted level for policy (the average of expenditures on sociocultural measures, investment in light industry and consumer goods, and wages) and for performance (the average of national income, consumer goods production, and housing production).

This use of speeches and resolutions raises a unique set of problems. Methodologically, there is no apparent effort to sample speeches systematically or to establish criteria for testing the hypotheses. In Bunce's work, there is a reliance on raw number of plenums or pages of text in speeches, with only a small effort to weight the substance of the rhetoric. Conceptually, it is not at all clear that speeches are a better measure of intentions than investments or budgeting expenditures. A Sovietologist should be particularly sensitive to the fact that words may be used as much to disguise an unpleasant reality as to reveal true intentions. Moreover, like actual expenditures, resolutions may be expressions of compromise among collective leaders rather than insights into the general secretary's intentions.

In fact, if one looks at the substance of Central Committee Plenums, one finds that many of the major decisions to improve the standard of living of the Soviet population came not in the initial years of succession, but in the years of consolidation. As Breslauer (1982, p. 278) points out, Bunce overlooks the "innovative reform packages" introduced by both Khrushchev and Brezhnev in the periods of their ascendancy.

Even if one accepts that counting Central Committee Plenums and legislative acts can measure intentions, the numbers appear to conform to the "consolidation" cycle rather than the cycle described by the original "succession connection" thesis. A closer look at the compendium of documents in *Zabota partii i pravitel'stva o blage naroda,* cited by Bunce, shows that the number of Central Committee decrees and legislative acts of the government was nearly twice as high in the consolidation period as in the succession period. Although in 1968 and 1969 these numbered 15 and 13, respectively, in 1965 and 1966 there had been only eight and seven. Moreover, although there were six Central Committee or Joint Central Committee-Council of Ministers decrees in 1968, there had been none in 1965. If number of legislative acts, and particularly the number of Party decrees, is an indication of intentions, then the consumer orientation of the regime would appear to have been higher in the years of Brezhnev's consolidation than in the preceding succession years.

Yet perhaps an even better indicator of changing intentions would be a comparison of the priorities in the five-year plans adopted during the succession period with those adopted later. For example, a comparison of the Eighth Plan, adopted in 1965, with the Ninth, adopted in 1970, not only fails to support the original succession connection, but appears consistent with the predictions of the consolidation connection summarized in Table 3.

According to the resolution of the XXII CPSU Party Congress in March, 1966, the purpose of the Eighth Plan was "to create the material and technical base of communism and to strengthen further the economic and defensive might of the country. . . ." Yet the Plan did not envision a break with established priorities. Schwartz (1982, p. 59) summarizes the CC Resolution by noting,

> One would normally expect the first congress of a new regime to act as a platform for the announcement of new directions. However, the resolution . . . was devoid of innovative approaches and policies.

Nor did the Plan envision a pumping up of the consumer sector. If anything, it suggested an elite orientation to policy in this succession period, making improvements in the "material and cultural living standards of the people" contingent on this (p. 63). Schwartz (1982, p. 59) comments,

> An improved standard of living and a convergence of the rates of growth of the capital and consumer goods sectors were to result mainly from increased labour contributions to economic production.

In contrast, according to the Resolution of the XXIV CPSU Congress on the Report of the Central Committee in March, 1971, the Ninth Five-Year Plan was to have a far greater mass-orientation. The Resolution summarized the purpose of the Plan by noting,

> The Congress fully approves the projected broad programme of social measures for the ninth five-year plan aimed at growth in the well-being of all strata of the population, the growing together of living standards of the urban and rural population, the creation of more favourable conditions for labour and leisure, for the all-round development of the talents and creative activity of the Soviet people, and for the upbringing of the rising generation.

The specific priorities of the Ninth Plan were 1) "to raise the real income of the population," mainly by increased wages and salaries as well as guaranteed minimum incomes; 2) "to provide for further growth of social consumption funds," particularly for welfare, public health services, education, and pensions; 3) to improve the quantity, quality, and variety of consumer goods; and 4) to expand and improve the quality of housing and leisure services (p. 177).

The mass orientation in the Ninth Plan is far more evident than in the Eighth. If anything, the "intentions" of the Brezhnev leadership seemed to show a far greater mass orientation in the period of consolidation than in the succession period.

The Anomaly of Agriculture

Agriculture is one important area that appears to show no discernible succession connection. This is significant because both Khrushchev and Brezhnev were so closely associated with this policy area. Moreover, much of the evidence adduced by Breslauer that led him to confirm the original succession connection thesis relates to agricultural policies. Yet on reexamination agricultural policy seems to confirm neither the original succession connection nor the predictions of the alternative consolidation connection summarized in Table 3.

Regressions of agricultural investment (policy outputs) on both sets of variables yield parameter estimates that are consistent with both sets of predictions, yet none is significant at the 0.05 level. Moreover, other evidence suggests that efforts to pump up the agricultural sector do not conform to the political pattern found in the nonagricultural sectors. For example, payments to *kokhozniki* rose 91% during the seventh five-year plan (during Khrushchev's challenged leadership), but only 46% during the next plan (during Brezhnev's succession and consolidation) (Russia, 1971, p. 383).

Similarly, statements of policy intent, specifically resolutions and speeches about agriculture, suggest that concern for agriculture does not conform to either political pattern. For example, in the Brezhnev era, serious attention to agriculture began with the first plenum after the removal of Khrushchev. The March, 1965, Plenum passed a resolution "On Urgent Measures for the Continued Development of Agriculture in the U.S.S.R." Land reclamation was addressed the following year in a resolution of May, 1966. Yet the attention to agriculture did not wane with the passing of the succession crisis. The October, 1968, Plenum adopted a resolution "On Progress in Implementing the Decisions of the XXIII Congress and Plenums of the CPSU Central Committee on the Question of Agriculture" and the July, 1970, Plenum adopted a resolution on "The Immediate Tasks of the Party in the Area of Agriculture." Moreover, even in the mid-1970s, Brezhnev introduced major programs for Non-Black-Earth Zone development, establishment of agro-industrial complexes, and grain importation to support expanded animal husbandry (cf. Breslauer, 1982, p. 277). Agriculture seems to be a near constant concern without any simple connection to either succession or consolidation.

Discussion

The evidence presented here does confirm the assertion that a Soviet leader "assumes different roles and advocates different priorities at different times" (Bunce, 1980, p. 967). As in the case of American administrations, "beginnings and endings are different" (Hess, 1977, p. 38).

The evidence presented here also is consistent with the assertion that one can, in part, explain shifts between mass-oriented and elite-oriented priorities by considering the political ambitions of Soviet leaders. In the Soviet Union, no less than in the United States, it seems safe to posit that "ambition lies at the heart of politics. Politics thrive on the hope of preferment and the drive for office" (Schlesinger, 1966, p. 1; cf. Mayhew, 1974).

Yet, the empirical results presented here fail to confirm the arguments that in succession crises Soviet general secretaries pump up mass-oriented policies. First, according to the hypotheses and empirical tests presented here, general secretaries are more likely to want to expand these mass-oriented policies *after* the succession—that is, during the early years of their ascendancy, as they seek to consolidate their hold on power. During the succession itself, the general secretary is more likely to want to expand elite-oriented policies.

Second, and this is an important addition to the original thesis, ambition alone is not enough to explain policy. One must also consider power and the resulting decision process. It is only *after* the succession that a general secretary is likely to have the power to effect a pumping up of either elite-or mass-oriented policy at the expense of the other. Taken together with the first amendment, this addition to the original succession connection thesis leads to a set of predictions about policy priorities that should be labelled the consolidation connection.

The results presented here, however, also suggest that not all policies conform to a single pattern. The most important anomaly is agriculture. Innovation in this area appears to be a near-perennial feature of post-Stalinist politics. It would appear that the pattern of innovation in agriculture does not conform to a simple succession pattern, and thus, when one speaks of a consolidation connection to policy, it will have to be limited to the nonagricultural sectors of the Soviet economy.

A second exception is incomes—notably wages, but also welfare (i.e., pension) expenditures, which appear to have conformed to the original succession connection pattern during the Khrushchev era, but to the consolidation pattern during the Brezhnev era. Incomes may be a significant anomaly in that increases in incomes can be among the most visible symbolic gestures on behalf of the mass and yet do the least to redistribute scarce resources away from elite interests. (The increase in incomes without a commensurate rise in actual consumer-oriented production was one of the causes of the strong inflationary pressures during the Stalin-Khrushchev succession.) Yet because an expansionary incomes policy can bring short-term popular support without forcing elite interests to forego significant resources, it may be an appealing gesture for a general secretary and collective leadership in a succession crisis.

This second anomaly suggests that there may be a need to distinguish between the political-priority patterns of distributive and redistributive policies. This possibility can only be verified by later research. Nonetheless, the pattern of results presented here suggests that the original succession connection thesis describes only policies without a significant redistributive impact on scarce budgetary and investment resources. The greater the redistributive impact of a policy, the more likely it is to conform to the consolidation connection presented here.

Not just policy outputs, but policy outcomes as well, conform to this consolidation cycle. Yet the effect of the consolidation period on the latter is less immediate and less consistent than its effect on the former. The cycle in outcomes lags about one year behind the cycle in outputs. The statistical tests of the relationship of consolidation to outcomes show a lower rate of confirmation than the tests of the relationship to outputs.

In addition, in the Brezhnev era joint statements of the leadership (i.e., resolutions and plans) conform to the consolidation cycle as well. Mass orientation is higher in the period of consolidation than in the succession crisis. Bunce (1981, p. 171) argues that successions are "a time of criticizing the status quo, generating new ideas and proving one's worth" (cf. Brown, 1982, p. 229). This is probably true. Yet it is the very diversity of opinion in these periods that leads not only to increased advocacy of consumer interests (e.g., Malenkov), but also to outspoken defense of the interests of the "steel eaters" (e.g., Molotov and Khrushchev) and to joint statements of policy that compromise these diverse interests and thus are less boldly innovative.

In short, the results presented here pose a major challenge to the original argument that the succession connection to mass-oriented policies in the Soviet Union parallels the electoral connection in western democracies. If there is a parallel to the electoral connection, it is not that described in the original thesis. Specifically, the succession struggle is not a honeymoon in the American sense; it is a campaign for office. In both systems, candidates try to outbid one another with *electorate*-oriented promises and policies. Yet the electorate in each is vastly different and so too are the policy appeals. If there is a Soviet honeymoon, it follows victory, as it does in the United States. It is in the period of the general secretary's ascendancy that he has adequate power to ignore his proximate electorate.

The results presented here also make suspect the claim that successions are "mechanisms of policy innovation." On the one hand, the findings suggest, as Burt has argued recently, that Soviet policies in periods of succession seem to be on "automatic pilot." On the other hand, as the cycle metaphor suggests, any effort to pump up either elite- or mass-oriented expenditures appears as a passing phase in a relatively stable longterm trend. After brief cyclical deviations, Soviet priorities return to their old patterns.

Although the results presented here reaffirm the tenability of some long-held notions about the Soviet Union, they also argue that it may be time to adopt greater methodological (specifically, statistical) sophistication than has been common in Sovietology. Sovietologists have too often relied for evidence on simple change measures without asking whether these changes are significant or simple random variation. In addition, as the re-tests presented here suggest, simple growth or change statistics may obscure more complex causal relationships. Specifically, long-term trends and demographic change in these tests proved to be important. Failure to account, or "control," for these and failure to use more rigorous tests of significance may lead to erroneous conclusions.

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"Two Nations": Class and Periphery in Late Victorian Britain, 1885-1910

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Structural equation modeling techniques test a series of hypotheses on the mobilization of class voting in late Victorian Britain. The enfranchisement of the working class triggers an organizational proliferation as parties seek to mobilize the new citizenry as well as a countermobilization of religious and territorial cleavages which divide the working-class vote along pre-industrial cleavage lines. Class voting appeared as early as the 1895 election as the newly enfranchised voters of 1884 supported the Labour and Lib-Lab parties. During the period the Liberals became increasingly isolated with their middle class, Nonconformist base as organized labor in particular moved to Labour and the Lib-Labs. Conservative support appears stable among middle-class and Catholic voters. Models of regional politics show Central England to evolve quite differently, with class voting less important than in the remainder of the country. In the periphery the Liberals appealed to the Celtic fringe, while Labour garnered the working class vote. The mobilization of the lower strata, which might have united class against class, generated a countermobilization, which divided the periphery against the center.

> Two nations, between whom there is no intercourse and no sympathy; who are as ignorant of each other's habits, thoughts and feelings as if they were dwellers in different planets; who are formed by different breeding, are fed by a different food, are ordered by different manners, and are not governed by the same laws . . . the rich and the poor.
>
> Benjamin Disraeli
> (Sybil, 1845)

The latter half of the nineteenth century and the first years of the twentieth witnessed a movement toward the full incorporation of Europe's working classes into political life. This development took place on two broad fronts: the gradual removal of institutional barriers to full citizenship, and the emergence of new organizational forms aimed at mobilizing the new citizenry. The politicization of the lower strata created the potential for extreme class polarization within societies as well as cross-national working class solidarity. However, the mobilization of class increasingly brought forth a countermobilization of the preexisting cleavage structures, so that by the end of the period, class mobilization had taken on a distinctive national character. The final outcome was the fracturing of European working-class politics along national lines and the shattering of hopes for cross-national class solidarity at the onset of the great war.

Past conceptualization and measurement of these concerns have seriously limited the findings and interpretations of class mobilization and the religious and ethnic countermobilizations by excluding, improperly conceptualizing and mismeasuring key variables. The following analysis uses a unique base and a promising new data analytic technique to overcome these difficulties and to explore three concerns at the nexus of the study of class politics: the formal incorporation of the working class, the salience of past territorial and ethnic struggles in conditioning class conflict, and the partisan mobilization of the polity. The research demonstrates the peripheralization of class politics in Britain, such that although politics became more similar across the periphery, it became more distinct from the center. These findings account for the limits of, as well as the uneven progress toward, a class-based polity.

The formal incorporation of the working class involved redefining the relationship of the individual and his economic status to the state while expanding the state's role in citizen formation. Political elites evolved complex suffrage schemes and social legislation designed to win the loyalties of the newly enfranchised classes. By isolating the worker from his peers and placing more of his

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social welfare and education in the hands of state institutions, the propensities for class solidarities could be dampened (Rokkan, 1962). In Germany, Bismarck hoped to domesticate the working classes through a combination of coercion and paternalism linked to a franchise scheme which disadvantaged the working class movements. In Britain Disraeli pursued a strategy of electoral and social reform coupled to appeals to the "one nation" ideal to win elements of the working class to Conservatism. Equally imaginative schemes were devised throughout Europe (Rokkan, 1970, pp. 147-168). The openness of the politically dominant strata to the demands of the newly enfranchised influenced their responses and these, in turn, the effectiveness of their incorporating strategies. For a recent restatement of this theme, see Lipset (1983).

The mobilization of the working classes, however, took place against the backdrop of prior ethnic, religious, and territorial struggles for unification. It was inevitable that some features of the previous cleavages would find expression in the class struggles of the period. Rokkan (1970, p. 100) notes that the "politics of cultural defense" in the periphery blunted the hard edges of class conflict in the south and west of Norway as well as the Celtic fringe of Britain. Where in addition political elites could strike a responsive chord among pre-industrial cleavages, they could divide the working class. In Germany the mobilization of the cross-cutting cleavages proved both effective and enduring (Claggett et al., 1982; Urwin, 1982a; Urwin & Aarebrot, 1981). The more salient the prior cleavages became, the less the likelihood of class solidarity and the greater the possibilities that class struggles would turn to secessionist solutions (Linz, 1973).

In addition to redefining the individual's relationship to the state and the historical legacy of past conflicts, the ability of elites to muster a countermobilization of the middle classes through the elaboration of mass party organization proved consequential (Lipset & Rokkan, 1967, p. 51; Weber, 1969, p. 105). After the democratization of the institutions of Scandinavia, political elites evolved organizational strategies to mobilize the citizenry in order to win control of the state (Svasand, 1980).

Late Victorian Britain provides an excellent setting to examine these dynamics. The reforms of 1883-1885 sustained the movement begun with the 1867 reform bill toward the incorporation and mobilization of the inarticulate strata into electoral politics. The Corrupt Practices Act of 1883 removed the most blatant means by which the economically dependent strata were manipulated by their superiors (O'Leary, 1962, pp. 159-178). The expansion of the franchise in 1884 and the redistribution of the seats in 1885 increased the electorate by 80% and brought greater balance between the counties and the boroughs. Inequities and anomalies of the previous reforms persisted however: plural voting, overrepresentation of rural districts, and difficult registration procedures. However, the reform marked a major step toward universal franchise and "one adult, one value, one vote," enlarging the electorate to include two-thirds of the adult males in England and Wales and three-fifths in Scotland. The reforms provided the opportunity for greater expression of class voting by reducing bribery and setting maximum electoral expenses, lowering thresholds to participation among householders and increasing the seats allocated to such industrial cities as Liverpool, Birmingham, and Glasgow (Blewett, 1965; Butler & Cornford, 1969, pp. 333-334; Jones, 1966; Matthew, McKibben & Kay, 1976; Seymour, 1915, pp. 415-518).

Expansion of the electorate also engendered organizational responses from political parties eager to mobilize the newly enfranchised as well as to counter similar attempts by their rivals. By the late 1870s the Conservative party organization, built by Disraeli in response to the previous franchise reform of 1867, had fallen on hard times. Under the guidance of Lord Randolph Churchill, a central organization and system of electoral agents was instituted. The Conservative reorganization sought to imitate the success of the Liberal organizational challenge which many scholars have pointed to as the prototype for all party apparatus: Chamberlain's Birmingham caucus initiated in 1868 (Cornford, 1963-1964; Ostrogorski, 1964; Tholfsen, 1959). In addition to the major parties, the Labour movement began to elaborate organizational strategies for the mobilization of the working class (Chamberlain, 1973; Cole, 1950, pp. 138-187; Moorehouse, 1973).

Popular politics in late Victorian Britain evidenced the stresses between social cleavages hearkening back to the pre-industrial struggles over national unification and the emerging class divisions. These tensions reflect the processes of nation-formation and state-building and may be conceptualized as a center-periphery conflict dimension (Hechter, 1975; Rokkan & Urwin, 1983; Urwin, 1980, 1982b). Such analysis concentrates on the territorial expansion and cultural domination of the center (England) over the Celtic fringe (Scotland, Wales, Ireland, and Cornwall) in British national development. The resulting cleavages emerge along economic, cultural, political, and territorial lines: economically advantaged and disadvantaged regions; language, religion, and ethnicity; political over or under representation, and boundary questions. Economic conflicts appear between agricultural and industrial interests, but also between workers and capitalists. Cultural cleavages are among established (Church of England) and nonestablished (Roman Catholic and Nonconformist) religions, as well as among language minorities (Gaelic speakers) and English speakers. The distribution of seats in Parliament and the differential size of district electorates form aspects of the political cleavage. All of these features may be territorially based, but in addition territorial boundaries form another source of tension (Rokkan, 1971; Torsvik, 1981; see Urwin, 1980, 1982b for a discussion of British development from this perspective).

Center-periphery tensions are very much in evidence in late Victorian Britain. Religious, regional, and ethnic issues found their expression in the Home Rule debates, particularly the Irish question. The Catholic population of Britain, particularly the Irish migrant, was attracted by the Conservative support for religious education, but the party's stance on Home Rule and established church repelled both the Catholics and Nonconformists. The Liberal support for Home Rule appealed to the Celtic fringe in general and the Irish in particular, but opposition to religious education and support for temperance alienated the Catholics, while gaining support among Nonconformists. Labour appeals to class struck a responsive chord among the growing industrial working class, but the party's stance on Home Rule and general antagonism to organized religion met resistance among more religious Catholics and Nonconformists alike. On the other hand, English Anglicans felt most comfortable with the Conservatives. Accentuated by the Educational Financing Act of 1870, the Irish question dominated much of British politics in the late Victorian era. In the early part of the period, when the Irish question was perceived as a religious division between established and nonestablished church, it found sympathy in Scotland and Wales, lending support to the Liberal party's Home Rule appeal. Later as the Irish question became transformed into a religions division of Catholic versus Protestant, Welsh and Scottish home-rule demands decreased. Until Irish independence, however, issues of religion, region, and language remained at the center of politics (Urwin, 1980, 1982b; Wald, 1983, pp. 162-201).

This tenacity of nation-formation and state-building questions muted the expression of class voting (Butler & Stokes, 1976, pp. 112-113; Clarke, 1971, 1974, 1977; Wald, 1978, 1982, p. 76). Moreover, despite the rise of class voting and weakening of the religious base of political life (Glaser, 1958, p. 363), the pertinacity of Home Rule issues in general and the Irish problem in particular, beclouded the political consequences of the enfeebling of the Nonconformist base of Liberalism.

There are three interpretations of the consequences of the rise of class politics for the continuance of territorial, ethnic, and religious identifications (Leifer, 1981, pp. 24-30; Ragin, 1979, pp. 620-623): developmental, reactive ethnicity, and ethnic competition arguments. The former would see the rise of class politics efface territorial, ethnic, and religious identifications which underpinned nineteenth-century political life, rendering, in its extreme, Disraeli's two nations or Marx's classes (Alford, 1963; Blondel, 1967; Butler & Stokes, 1976, pp. 109-121; Cornford, 1963-1964, pp. 64-65; Lipset & Rokkan, 1967, pp. 19-21; Pelling, 1968, p. 120; Pulzer, 1967, pp. 46, 98). The reactive ethnicity argument stresses the impact of uneven class development combined with the continuance fo territorial, ethnic, and religious identifications engendering a "cultural division of labor" (Hechter, 1975, p. 30) and a peripheralization of the Celtic fringe, leading to "reactive ethnic cleavages" (Gellner, 1974, pp. 147-178; Hechter, 1975, pp. 15-64; Ragin, 1977). Two results are possible: heightened ethnic consciousness among the peripheral working class where economic inequalities, intracollectivity communication, and intergroup differences of culture are greater (Hechter, 1975, p. 43). A second possibility is the political expression of heightened ethnic awareness among the middle classes finding its expression in counter-center political movements (Ragin, 1977, p. 449). The ethnic-competition argument holds that economic development and inequalities will generate political cohesiveness across classes within ethnic groups as these groups become more aware of their economic subordination to the center (Deutsch, 1953; Hannan, 1979; Wallerstein, 1974, p. 353). The result would generate high cohesiveness within each ethnic group unfettered by class divisions as the groups became more distinct from each other.

Late Victorian Britain is a period of transformation from a polity predominantly based on cultural, territorial and religious cleavages to one based on class divisions. The full expression of class politics, of course, was not articulated until the inter-war period when, after a further expansion of franchise in 1918 and the removal of the "Irish Problem," Disraeli's second nation was fully incorporated into the political community and the dominant issues of the late Victorian period receded. The 1885-1910 period, however, introduced the institutional changes which incorporated significant portions of the politically inarticulate strata, thus providing opportunities for the mobilization of the previously disenfranchised, an opportunity that generated new forms of political organization. These developments set the stage for the full incorporation after the war.

Previous examinations of late Victorian Britain yielded conflicting or incomplete analyses of the persistence of pre-industrial cleavages and the salience of class conflict. Table 1 summarizes the principal features of the major empirical research on this problem. Five studies are of particular interest when discussing electoral politics of the late Victorian era: Hechter (1975), Ragin (1977), Leifer (1981), and Wald (1982, 1983). All deal with the pre-World War I period, all use systematic data for statistical analysis, all approach the problem of shifting cleavage structures, and all conclude that pre-industrial cleavages coexisted to some degree with the emerging class politics of the period. The studies differ in their analysis of the regional responses to class mobilization. All agree on the presence of anti-Conservatism outside England, but disagree on the social base and vehicles of its expression.

All the studies, however, suffer flaws of conceptualization, measurement, design, and data analysis which limit their understanding of class and center-periphery conflicts. The major flaw shared by all the studies is the conceptualization and measurement of the dependent variable as a single dimension of Conservatism/Anti-Conservatism. Using this dimension has two major deficiencies: It suggests the Conservatives were the nationalizing party primarily based on appeals to class, and second, it collapses class and center-periphery conflict into a single dimension and does not permit a distinction between them. Therefore, it biases the results in favor of the reactive ethnicity argument, because either differential class or reactive ethnicity voting could produce regional differences interpreted as anti-Conservatism. The analysis, therefore, not only develops a faulty test between the developmentalist and reactive ethnicity arguments, but cannot test the ethnic competition argument, which suggests class unity within ethnic groups is problematic. The difficulty is overcome by including all parties, particularly the Lib-Lab and nascent Labour votes. Minor parties and factions of the Liberals were regional or local and thus are better indicators of regionalism, whereas Lib-Lab and Labour are better indicators of class voting by the working class.

None of these studies can analyze the incorporation and mobilization of the working classes or the countermobilization of the middle classes and pre-industrial cleavages because they exclude enfranchisement and nonvoters. Because voter enfranchisement and participation varied across time, space, and social class, analyzing differential mobilization is crucial, yet only Wald mentions this variable, but he does not include it in his equations. Hechter seriously mismeasures this variable (compare Hechter, 1975, p. 221, and Wald, 1983, p. 209). Enfranchisement could form a crucial barrier to the expression of both class and center-periphery conflict, because registration discriminated against key groups, for example, unskilled working class and Catholics.

Similar problems are posed by the independent variables. Hechter and Leifer mix class and economic marginality by using industrialization as a surrogate measure of both. Ragin and Wald overcome some of these flaws by using occupational groupings, but only Wald uses trade unionism as a key expression of class. All use religious and language variables as measures of peripheralism. However, only Wald has access to multiple indicators, but not the data analytic techniques to exploit them fully. Thus, all the studies assume the observed variables are isomorphic with the concepts they are thought to measure (Bentler, 1980, p. 434; see also Appendix).

Three of the studies—Hechter, Ragin, and Leifer—use county level data aggregated to correspond to parliamentary districts. The difficulties are several (Page, 1978; Steed, 1979), including changing boundaries, large heterogeneous units, and barriers to cross-time analysis. Wald's data are richer and based on constant units of analysis across time, but he does not use cross-time dynamic techniques. Hechter, Ragin, and Wald use regression analysis, usually step-wise, whereas Leifer uses discriminant analysis (see the Appendix for a discussion of the limits of regression analysis, also see Franklin & Mughan, 1978, on the limits of discriminant analysis). All compare coefficients across elections; thus none can distinguish between trends and fluctuations in voter mobilization and alignments across time.

This analysis differs from the above studies in the following respects: the inclusion of minor parties, abstentions, and enfranchisement; the introduction of cross-time dynamics; and the use of a new data analytic technique able to make maximum use of multiple indicators to develop structural equation models of the electoral dynamics of the period. Latent variable path analysis (LVPA) fully exploits multiple indicators by developing latent variables that are more valid, reliable, and closer to the concepts they seek to measure. The introduction of cross-time dynamics enables the model to account for both stability and change across elections. The logic is similar to the introduction of lagged endogenous variables for the elections of 1895, 1906, and 1910: The path coefficients between the lagged variable and its counterpart represents the variance accounted for by the previous election; thus the remaining variance represents change from the preceding elec-

Table 1. Principal Findings of Empirical Research on Social and Regional Basis of Politics in Late Victorian Britain

			Variables			
Study	Period	Data	Independent	Dependent	Design	Results
Butler & Stokes, 1974	Pre-1918 to 1966	Survey	Religion. Nonman, region	Major party vote	CS/ cohort	Variations on a theme of class (p. 84)
Hechter, 1975	1885-1966	E counties	Nonconf. Indust.	Conservative party	CS	Persistence of sectionalism (p. 264)
Ragin, 1977	1885-1966	E counties	Nonconf. occupation, region	Conservative party	CS	Peripheral anti-Consv., but Labour not an outlet for sectionalism (p. 448)
Miller, 1977	1918-1974	E CUA	Religion. nonman. region	Conservative Liberal Labour vote	CS	Labour and Liberals expressions of peripheral status, Conservative center (p. 222)
Leifer, 1981	1885-1966	E counties	Religion. Language, Eco Devel	Conservative Liberal vote	CS	Class more important in dominant regions (p. 38); no support for reactive ethnic model (p. 41)
Wald, 1982	1885-1910	E CUA	Religion. occupation, trade unionism	Liberal and/or Labour vote	CS	"Highly organized trades had a significant positive effect on the left vote" (p. 76)
Wald, 1983	1885-1910	E CUA	Religion, occupation, trade unionism	Conservative Unionist vote	CS	"Anti-Conservative sentiment outside England" (p. 154)

E: Ecological
CUA: Constant units of analysis
CS: Cross-sectional design
Nonman: Nonmanual workers
Nonconf: Nonconformism

tion, some of which is accounted for by the religious and class-latent variables.

These features enable the analysis to go well beyond the previous research, which concluded only the existence of anti-Conservatism in the periphery. Anti-Conservatism will show itself to have two faces: a working-class vote for Labour and Lib-Lab, and a religious vote for the Liberals. Moreover, working class voting is stronger in the periphery than in the center, dividing the periphery against the center and making politics more similar across the periphery as it becomes distinct from the center. Mobilization and countermobilization play crucial roles in the emergence of class voting, particularly in the periphery, but also in the continued success of the Conservatives in Central England.

Hypotheses

The reforms of 1883-1885 generated a reservoir of potential supporters who appear in the early years of the period as nonvoters; over time, however, the partisan mobilization incorporates nonvoters into party alignments. At the beginning of the period religious and ethnic variables are better predictors of party vote; by the end of the period, class variables increase their predictive capabilities. Working-class association with the Liberal party declines, while increasing with the Labour and Lib-Lab parties. The mobilization of the working class gives rise to a countermobilization of the middle classes and pre-industrial cleavages.

A comparative analysis of the regional dynamics in association with the changing social base of politics yields evidence to support one of several hypotheses: If class is a cross-regional binding force, then regional differences in partisan politics diminish over time as class becomes more important than religious and ethnic ties (developmental hypothesis). If working-class grievances combine with ethnic consciousness enhancing regional distinctiveness and class consciousness (cultural division of labor argument), then class stratification does not generate political homogenization and regional peripheralization of the working class results (reactive ethnicity hypothesis). If religious and ethnic loyalties unite social classes to enhance regional distinctiveness, then an alternative form of peripheralization results (ethnic competition hypothesis). The reactive ethnicity and ethnic competition arguments differ on the hypothesized relations across classes within ethnic groups; the former hypothesizes class divisions within ethnic groups, the latter across-class cohesion within ethnic groups. Both suggest increasing distinctiveness of the periphery as a consequence of class mobilization. Finally, if class politics does not emerge while religious and ethnic ties remain strong and regional distinctiveness persists, traditional politics prevail (null hypothesis).

Concepts, Measures, and Data

The focus of the analysis now becomes the concepts of class, religion, stable and unstable partisan voting alignments across time and territory, the latter capturing the center-periphery dimension. Religion and class form the independent variables in the analysis. Religion signifies more than religious affiliation, however; religion and its association with ethnicity, territoriality, and language formed the social base of political life in late Victorian Britain, finding its expression in the Home Rule issues of the period. The concept of class, of course, has been subject to multiple interpretations and measurement (Wald, 1982). In contrast to Miller (1977) and Butler and Stokes (1976), who use bi-class measures and Wald (1983) and Ragin (1977) who use discrete occupational categories, this analysis uses occupational and religious measures[1] to construct class and religious latent variables. The relationship of religion and class to the stability and variability of partisan alignments across time is the centerpiece of the analysis. Party vote and abstentions construct latent variables of stable and variable partisan alignments. Models are constructed and tested using latent variable path analysis (LVPA).

LVPA is a general class of data analytic techniques that combines path analysis with the use of latent variables. The technique used here is latent-variable path analysis with partial least squares (LVPLS). Latent variables (LVs) are theoretical constructs which cannot be observed directly but which are measured by observed or manifest variables (MVs). In this sense the conceptualization is similar to factor analysis. Each manifest variable consists of a unique as well as common variance with other manifest variables thought to underlie the theoretical or latent variable. The first task then is to construct models of the latent variables and test their reliability on the observed or manifest variables (Bentler, 1980; Lohmoeller, 1982a; McArdle & McDonald, 1981).[2] See the Appendix for a discussion of this technique.

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[1] Data consist of 115 surrogate constituencies for the eight general elections between 1885 and December, 1910 constructed by matching the parliamentary constituencies to census units for 1891. Multiple indicators for religion and ethnicity derive from the 1851 census, whereas occupational indicators derive from the 1891 census. Trade union density is calculated for 1888, 1901, and 1910. Electoral data consist of the vote for all major and minor parties as well as nonvoters. Because of a paucity of Labour candidates, "Labour" in the data includes the Independent Labour Party, Labour Representation Committee, and other Labour-affiliated groups. Wald used the Lib-Lab designation for Liberal candidates with trade-union background who had no affiliation with Labour groups. Finally, Labour candidates running with Liberal candidates in two member districts were classified as Labour. This analysis uses only the elections of 1885, 1895, 1906, and January, 1910. More or less regularly timed elections facilitated the across-time analysis. The January, 1910 election was more typical of the period and less representative of the crisis of Peers vs. People of the December, 1910 contest. A fuller description of the data and its construction is found in Wald (1983, pp. 74-88).

The 1851 and 1891 censuses provide the manifest variables for constructing the latent variables for religion. For the three major religious groupings of Church of England (C of E), Nonconformism (NON), and Roman Catholic (RC), the 1891 census provides clergy per capita; the 1851 census generates the maximum attendance at the best-attended service per capita (MAX); number of persons attending day school per capita (DSPCT); number of persons attending Sunday school per capita (SSPCT). Manifest variables for ethnicity are Welsh (WAL), Scottish (SCOT) and Irish (IRE) born males per capita. Class measures include: CL2—proportion of the 1891 economically active population in Class 2 of the 1951 occupational grading scheme; SKILL11—proportion of the economically active male population engaged in skilled occupations defined by the 1911 Registrar General's scheme; MANLAB—proportion of the economically active population classified as manual laborers, 1891; INDLAB—proportion of the economically active population classified as industrial or mining, 1891; TUDEN, 88, 01, 10—proportion of the major occupational sectors—transport, printing, building, textiles, wood, clothing, mining and metal—organized in trade associations in 1888, 1901 and 1910; NAGLAB—all forms of general unskilled labor in non-agricultural employment, 1891; AGRLAB—general unskilled agricultural labor, 1891.

Table 2 defines the abbreviations used in Figures 1 and 2 to represent socioeconomic and religious variables. The final two-digit number in each variable indicates the year of observation.

Figures 1 and 2 follow the notational convention for LVPA: Manifest variables are represented by squares, latent variables by circles, directionality by straight arrows, undirected covariance by curved arrows, and unexplained variance by the smaller circles on the manifest or latent variables.

Figure 1 presents the LVPLS estimates for the manifest-to-latent variable parameters for religion and class. The lowest loading is between the manifest variable. Church of England Sunday School attendance per capita, 1851 (CESSPCT51), and the latent variable, Church of England (C of E) with a value of .446. The manifest variable proportion of Scottish-born males in 1891 does load not positively and was dropped from the model.

Shifting Voter Alignments, 1885-1910

Figure 2 represents the results of the LVPLS algorithm for the entire polity between 1885 and 1910; regional differences are not included, and the full model is used as a backdrop against which regional variations are highlighted. Even without regional variations this initial model provides much information on the dynamics of incorporation, mobilization, and the social base of political life.

The figure presents all directed paths in the model having an absolute value of .2 or greater. Although presented in four parts, the model must be conceptualized in its entirety as a single statement on the dynamics of voter alignments. All parameters in the model are generated by an integrated, iterative procedure in which the algorithm seeks a partial least-squares solution according to two criteria: 1) the LVs must be good predictors, or if targets, predictable; and 2) if outward-directed relationships are specified, then the LVs must be good predictors of the MVs. In Figure 2 the LVs on the left side are outward directed, generated by principal components analysis and constrained to fit their specified position in the inner or path model. The LVs on the right side are inward directed, generated by canonical correlation, and the weights are the best standardized linear combination of scores to fit the relationships specified by the path model.

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[2] The estimation algorithm is latent variable path analysis with partial least-squares (LVPLS) (Lohmoeller, 1981). LVPLS has several advantages for social science research: It is statistically robust, yet less knowledge sensitive than some alternative methods. The technique allows for two methods of estimating relationships between the manifest variables (MVs) and latent variables (LVs): inward and outward directed. If the researcher believes the manifest variables are common measures of an underlying theoretical construct, outward directedness is appropriate and the coefficients are constructed by principal components factor analysis. On the other hand, if the researcher believes the manifest variables have no known relationship to a latent variable, inward directedness is assumed and the coefficients are calculated by canonical correlation.

The analysis constructs LVs for class and religious as well as stable and unstable vote variables. The latter are in accordance with the conceptual definition of realignment as shift in voting patterns from stable to unstable and a return to stable patterns. LVs have several advantages over the use of MVs or observed variables. No information is discarded nor is any new information created by the use of LVs; however LVs include distinct advantages over observed or manifest variables: LVs with multiple indicators are more reliable drawing upon the common variance of the MVs. Second, LVs are likely to be closer to the theoretical concepts they seek to measure. Third, LVs permit the distinction among common variances, unique variances and error terms, the constituent parts of observed variance. Thus information is not discarded but reallocated to more theoretically meaningful categories.

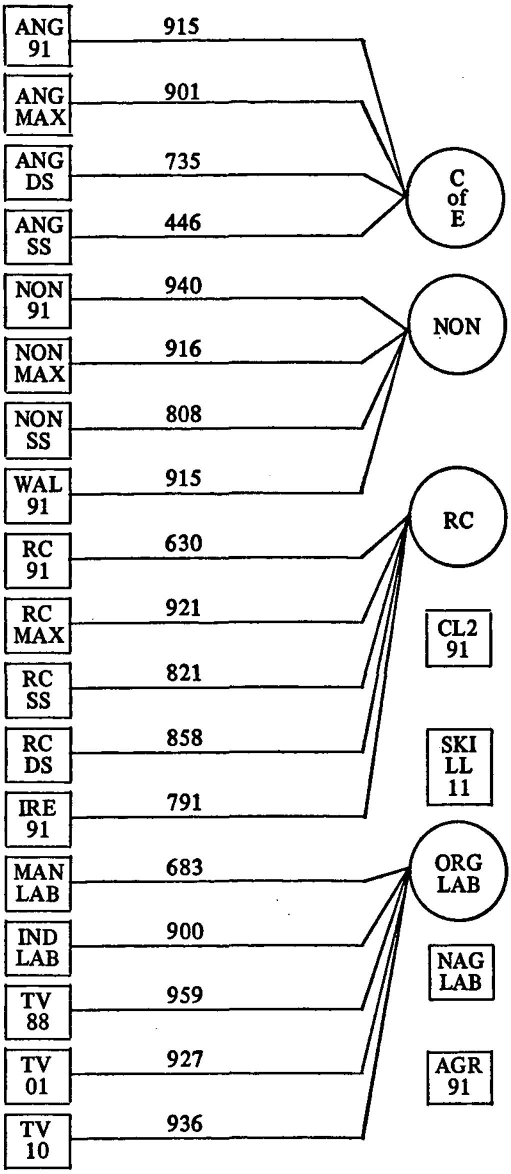
Construction of the class and religious latent variables proceeded by the following steps: First, a principal components factor analysis defined the manifest variables into latent variables within the religious and class groupings. Second, outward directedness was assumed in the parameter estimation in the LVPLS model. The assumption is that the observables reflect the underlying or theoretical dimensions of religion or class. Latent variable construction for party voting followed these steps: First, a single principal component factor was constructed of the percentage party vote and nonvoters across time; high loadings across time indicated stability, low loadings indicated instability. In other words, each of the vote variables was correlated with itself in each of the elections; high loadings (defined as greater than .6) indicated that the percentage vote remained stable for that party across time and that the party vote in the first election was a good predictor of the same party vote in the second. These loadings were the selection criterion for the latent variable blocks in the LVPLS model. Voting blocks form a dimension of choice available to the citizenry, one of which is abstention. Crewe (1974) notes the importance of including nonvoters in the analysis of electoral change. Therefore the MV-LV parameters are inward directed, and the LVPLS factor weights are the estimates. Conservative and Liberal parties load highly on the stable vote latent variable (STV-LV); all other party vote and nonvoters load on the unstable vote latent variable (UNSTV-LV). The inward directed parameter estimates may be thought of as scores of predictability; the higher the score, the greater the predictability. If two scores are high, but of the opposite sign, they indicate a dimensionality anchored by the parties as competitors. Changes in the values and signs across time indicate shifting voter alignments according to the paths coming from the religious, class, and preceding vote latent variables.

The model rejects the null hypothesis since working-class voting finds expression outside the traditional partisan alignments; however, addi-

Table 2. Abbreviations Used in Figures 1 and 2

Abbreviation	Variable Description
Manifest Variables	
ANG91	Anglican priests per capita
ANGMAX51	Maximum Anglican church attendance per capita, 1851
ANGDSPCT51	Anglican dayschool attendance per capita, 1851
ANGSSPCT51	Anglican Sunday school attendance per capita, 1851
NON91	Nonconformist clergy per capita, 1891
NONMAX51	Maximum nonconformist church attendance per capita, 1851
NONSSPCT51	Nonconformist Sunday school attendance per capita, 1851
WAL91	Welsh-born males per capita, 1891
RC91	Roman Catholic priests per capita, 1891
RCMAX51	Maximum Roman Catholic church attendance per capita, 1851
RCSSPCT51	Roman Catholic Sunday school attendance per capita, 1851
RCDSPCT51	Roman Catholic day school attendance per capita, 1851
IRE91	Irish-born males per capita, 1891
CL291	Proportion of the 1891 economically active population in Class 2 of the 1951 occupational grading scheme
SKILL11	Proportion of the economically active males in skilled occupations defined by Registrar General's scheme, 1911
MANLAB91	Proportion of the economically active population classified as manual laborers, 1891
INDLAB91	Proportion of the economically active population classified as industrial laborers, 1891
TU88,01,10	Proportion of the major occupational sectors—transport, printing, building, textiles, wood, clothing, mining and metal—organized in trade associations, 1888, 1901, 1910
NAGLAB91	Proportion of all forms of general, unskilled labor in nonagricultural employment in the economically active population, 1891
AGR91	Proportion of the general unskilled agricultural labor in the economically active population, 1891
Party and Electoral Abbreviations Used in Figure 2	
Manifest Variables	
NV	Non-voters
CON	Official Conservative
CEN	Liberal Unionist
LIB	Liberal
LAB	Labour, including LRC, ILP, SDF
LLAB	Lib-Lab
ICON	Unofficial Conservative
ILIB	Unofficial Liberals
OTH	Other
ENFR91	Estimated proportion of the male population 21+ years on the electoral register, 1891
Latent Variables	
C of E	Church of England
NON	Nonconformism
RC	Roman Catholic
ORGLAB	Organized Labor
STB	Stable Vote
UNSTB	Unstable Vote

Figure 1. LVPLS Principal Component Factor Loadings of MVs to LVs for Religion-Ethnicity and Class ($N = 79$)



tional models are necessary to discriminate among the remaining hypotheses. The salient findings rejecting the null hypothesis may be summarized as follows: The incorporation and mobilization of the working class into partisan alignments as well as the countermobilization of religious cleavages engenders three partisan groupings: the Conservatives, appealing to religion and class; the Liberals, benefiting from Nonconformism; and Labour-Lib-Lab, gaining from the working class vote, particularly organized labor. Thus, nascent class divisions cross-cut religious cleavages functioning to divide further the polity to the disadvantage of the Liberals. Supportive evidence from the model includes: first, the political incorporation of the newly enfranchised as a gradual process evidenced by the decline in coefficient associated with the nonvoters MV; second, the partisan incorporation of the newly enfranchised indicated by the decline in the coefficients for the minor and "other" parties and the emergence of three partisan groupings—Conservatives, Liberals, and Lib-Lab-Labour. Independent Liberals, Independent Conservatives, Liberal-Unionists, and "other" parties diminish across the four elections. Third, the level of enfranchisement has no effect on the parties' electoral fortunes, although rural and Nonconformist voters are over-represented (see Butler, 1968, p. 5). Fourth, class voting appears, but not to the exclusion of religious influences. The Conservatives retain the support of the middle class and gain from the shift in the Catholic vote; the Liberals lose the support of the working class, particularly organized labor and rural workers, but gain from Nonconformism, whereas Labour-Lib-Lab gain from working class voting, particularly organized labor.[3]

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[3] The model is, of course, ecological. Care is necessary not to commit the ecological fallacy by inferring individual behavior from psephology. A second limitation is the possibility of contextual effects on the model's relationships, that is, it is not Catholics voting Conservative, but non-Catholics reacting to Catholics. The limit could be compounded by the impact of suffrage restrictions. The latter difficulty is overcome by use of the franchise variable which shows no discriminatory effects for Catholics. The contextual problem is lessened because LVPLS partials out the effects of other variables: At all levels of Catholicism, Catholicism is associated with Conservative vote.

Moreover, the model in Figure 2 is based on 79 observations for which there are complete data. The number of observations and the number of parameters raise questions of stability and reliability of the estimates. This problem is mitigated by several features of the research. LVPLS works with a smaller number of zero intercorrelation assumptions between the residuals and the variables consistent with its exploratory techniques and the theoretical underpinnings of this research. Second, when the LVPLS models are tested with ML techniques, additional parameters are freed and tested. Third, these latter tests take account of the varying degrees of freedom in the chi square/degrees of freedom ratio as a measure of goodness-of-fit. Finally, because the total observations are 115, it is desirable to know how well the model in Figure 2 could reproduce an identical model using pair-wide deletion of missing values. The means available to calculate goodness-of-fit statistics are discussed below. Although the substantive

The LVPLS algorithm generates a robust least-squares solution close to the data, but does not provide goodness-of-fit statistics. The stability and statistical significance of the least-squares solution can be tested with maximum likelihood (ML) techniques.[4] The results of these tests confirm the LVPLS solution. The model is stable, and the coefficients analyzed are significant.

Regional Variations in Voter Alignments, 1885-1910

Figure 2 presents the electoral dynamics at the national level, but does not permit direct analysis of the impact of class mobilization on the center-periphery dimension. The hypotheses concerning the interplay of pre-industrial and class cleavages can be tested by comparing regional models of the processes in Figure 2. Best-fitting models are constructed for subsets of the data using a jack-knife technique; then the results of each model are used to predict all other models.[5]

The differences across the models are summarized in Table 3, which presents the goodness-of-fit statistical indexes generated by maximum-likelihood methods when the path model of one subset is used to predict another.[6] The question

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results of the models are quite similar, these tests show a poor fit between the two models expressed by a chi-square/degrees-of-freedom ratio of 2.63 and Z-value of 6.75. Caution requires that the model not be generalized to the entire polity. Since most of the missing data occurs because minor parties are not present, the units used in the models are the more dynamic ones and thus may overrepresent political change. Urwin (1982b, p. 41) suggests higher party competition is a measure of the nationalization of British politics during this period; therefore, the model would tend to underestimate the persistence of traditional politics.

[4] Whereas LVPLS is conservative by constraining its solutions close to the data, ML solutions assume the model is valid and provide a goodness-of-fit statistics of the model to the covariance matrix of the MVs. In addition ML solutions generate standard errors for the coefficients. The results obtained by the ML solution here are best thought of as restricted regression solutions in that the variances and covariances of the LVs on the left side of the model are fixed at their LVPLS values while the path values and the covariances on the right-hand side are started at their LVPLS values and permitted to change. In the following test only the inner or path model is examined, in other words, the loadings and weights of the MVs on the LVs are also fixed at their LVPLS values.

The testing procedure is to fix various parameters with high estimates at zero and examine the changing values of the residuals, the standard errors of the coefficients, and the chi-square/degrees-of-freedom ratio. The second test fixes parameters estimated to be near zero at zero and tests for the same goodness-of-fit statistics (James, Mulaik, & Brett, 1982, pp. 91-95). The results confirm the LVPLS solutions: The fixing of near-zero coefficients at zero does not cause a deterioration of the fit of the model, but the fixing of a high coefficient at zero does. Moreover, it is not possible to force the explained variance of high parameters into low parameter estimates: Some key parameters could not be increased in value; for example, the near-zero path between stable vote LVs in 1895 and 1906 was not increased by altering the values of other coefficients, lending greater confidence to the statement that a realignment occurred between those two elections. All coefficients analyzed above remained stable and are statistically significant.

[5] The first step is the generation of regional data sets. However, because each region of the United Kingdom does not possess adequate cases to provide stable estimates of coefficients, a version of the jackknife technique generates subsets of the data by the removal of one region from each subset. The result is the full data set modelled in Figure 2 ($N=79$), as well as subsets of non-Central England ($N=51$), non-Scotland ($N=71$), nonperipheral England ($N=48$), and non-Wales ($N=67$). Two regions, Central England ($N=28$) and Peripheral England ($N=31$), provide marginally adequate case numbers for stable estimates. See Wald (1983, p. 143) for constituent units of the regions.

The second step is the construction of the best fitting LVPLS model for each subset. Each step in the model construction described for Figures 1 and 2 is repeated for each subset. The results for the latent variable construction indicated some immediate support for the homogenization hypothesis: The MV-LV relationships for the class LVs are consistent across subsets, whereas the religion and ethnic variables are not. However, the construction of the stable and unstable vote LVs for Central England produce markedly different patterns from those in the other regions. In Central England the principal component factor analysis assigns Conservative and Liberal-Unionist vote to the Stable LV. The Liberal-Unionists split with the Liberals over the Irish Home Rule policy of Gladstone in 1885 when Chamberlain rejected Parnell's demand for an Irish parliament (Blumer-Thomas, 1965, pp. 133-141). The best-fitting model for Central England has Conservative and Liberal-Unionist vote MVs loading on the stable vote LV and all other MVs loading on the unstable vote LV.

[6] The logic of the tests used here is to examine how closely one model could reproduce another. The models are comparable in the number of variables and the path structure among the variables. The models are then used to predict one another generating a series of goodness-of-fit measures (see James, Mulaik, & Brett, 1982, pp. 83-95 for an explication of the logic). Table 3 is read as column input to rows, that is, the model for the subset in the column is used to predict the model for the subset in the row. The predictions are generated in both directions so that each model predicts each other model. Each off-diagonal cell contains two entries: McDonald's (1975) chi-square/degrees-of-freedom ratio and Bishop, Fienberg, and Holland (1975) Z-value. The results of the technique would be biased by the group means of each subset; therefore, the statistical tests are

Table 3. Goodness-of-Fit Measures Across Regional Policy Models

	FULL	NCENG	NWAL	NPENG	NSCOT	PENG	CENG
FULL	—	1.6[a] 3.1[b]	69.1 54.7	54.2 49.1	4.0 10.4	15.4 26.3	110.5 67.0
NCENG	2.5 6.4	—	44.4 44.8	55.0 49.5	7.0 16.2	63.3 52.7	108.0 66.3
NWAL	81.7 58.9	33.7 39.3	—	98.9 63.9	78.2 57.8	36.1 40.7	85.9 60.2
NPENG	90.0 61.4	58.6 50.9	138.9 73.7	—	72.9 56.0	58.0 50.7	250.9 78.1
NSCOT	4.5 11.4	5.0 12.6	73.7 56.3	48.9 46.9	—	12.1 22.9	111.9 67.3
PENG	40.2 42.8	24.5 33.5	79.5 58.2	91.0 61.7	28.2 36.0	—	125.7 70.7
CENG	101.0 64.5	61.0 51.8	96.7 63.3	136.5 61.0	102.9 65.0	60.6 51.6	—

FULL: Full data model
NWAL: Non-Wales
NSCOT: Non-Scotland
CENG: Central England
NCENG: Non-Central England
NPENG: Non-Peripheral England
PENG: Peripheral England

[a] Chi-square/degrees-of-freedom ratio (McDonald, 1975).
[b] Z-Value (Bishop et al., 1975).

asked is: How accurately can the path model derived from one subset of the data reproduce the inner or path model of another subset (James, Mulaik, & Brett, 1982, pp. 83-90)? The results might suggest several possible outcomes: If there are insignificant differences across the models, then regional variations are unimportant and the dynamics of Figure 2 represent the addition of class divisions to existing cleavages and the nationalization of politics along those cleavage lines. The finding would discount regional differences at the same time it would modify traditional politics. The result would suggest a variation of the developmental argument with the persistence of pre-industrial cleavages and the addition of class divisions. Second, if each peripheral region becomes more distinct from the others as well as the center, then regional peripheralization results. Further examination of the dynamics within each region would confirm or discount the reactive ethnicity or ethnic competition hypotheses, depending on the presence or absence of cross-class alliances within regions. Third, if politics becomes more similar across the periphery at the same time it becomes more distinct from the center, then cross-regional peripheralization results. Examination of within-center and periphery models would evaluate the reactive ethnicity and ethnic competition hypotheses.

The results in Table 3 clearly reject the developmental hypothesis, even in its modified form: regional differences remain major distinguishing features of politics in late Victorian Britain. The best predictor of the full model in Figure 2 is non-Central England. Within the periphery, however, the removal of Scotland provides the next best prediction to both the full model and to the non-Central England model. In other words, the deepest cleavage lies between Central England and the remainder of the country. Central England is the most distinctive regional polity, whereas Scotland is the most distinctive regional polity in the periphery. Further support for this interpretation

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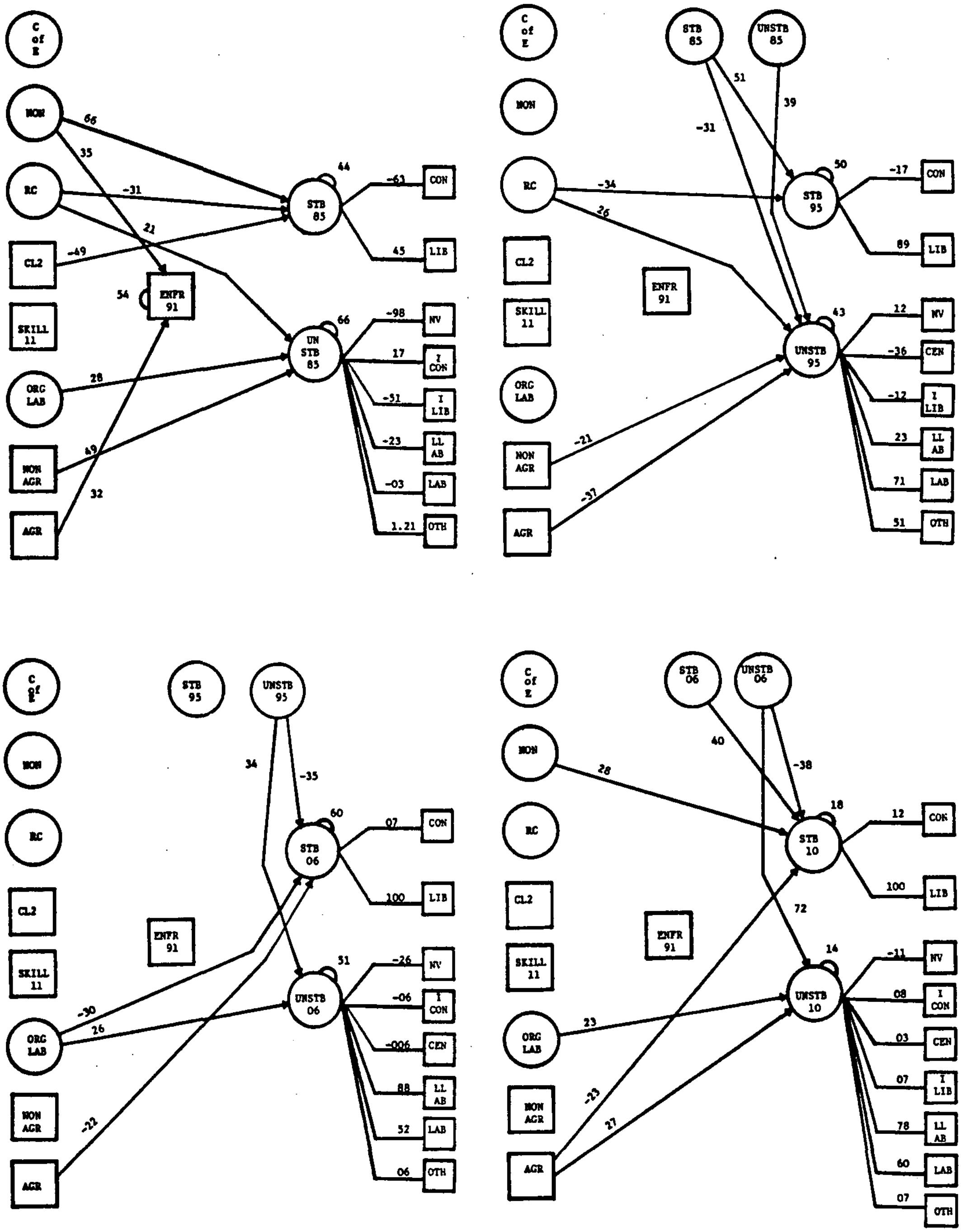
computed with and without group means. Table 2 in the text includes group means, but similar results are achieved after removing group means. The overall fit of the models deteriorates within the removal of the group means, but the ordering of the relative fits remains similar across the models. All statistical measures of goodness-of-fit have their limitations, including those reproduced in Table 2. The chi-square measure is sensitive to sample size which with small samples leads to the acceptance of the model to the data (James, Mulaik, & Brett, 1982, pp. 151-153). In the current analysis this is an obvious limitation; thus the chi-square/degrees-of-freedom ratio provides only relative fit of the models to each other. The *Z*-value is independent of sample size; it uses the degrees of freedom in an effort to reduce influence of sample size. A fuller discussion of these tests can be found in Horn and McArdle (1980) and Tucker and Lewis (1973).

Figure 2. Latent Variable Partial Least Squares Path Model for Religious-Ethnicity and Class Latent Variables to Voting Latent Variables, 1885-1910 ($N = 79$)

Presented in four separate diagrams, one for each election.
Note that only paths with absolute values of .2 or greater are shown.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is provided by the worst-fitting predictions between Central England and non-Peripheral England and between Central England and Peripheral England. These findings suggest that the emerging class politics in late Victorian Britain united the population across the peripheries at the same time it heightened the division between the center and the periphery. The results indicate the cross-regional peripheralization of politics.

The focus of the analysis now becomes within center and periphery dynamics to distinguish between the two remaining hypotheses—reactive ethnicity and ethnic competition—yielding insights into the dynamics of both the center-periphery and class mobilizations. The non-Central England model is a caricature of the full model: The Liberal party becomes increasingly dependent on nonconformism as it loses support of organized labor, agricultural labor and Roman Catholicism which shift to the Lib-Lab and Labour. The Conservatives retain support from Catholicism and the middle class. These findings support the reactive ethnicity argument but not that offered by Hechter, Leifer, or Wald. Although all four authors found anti-Conservatism in the periphery, only two offered further interpretations: Hechter (1975, pp. 272-273) hypothesized the cultural division of labor to unite the working class grievances with ethnic consciousness to enhance regional distinctiveness finding its expression in the Labour movement. Ragin (1977, p. 449) speculated that reactive ethnicity would develop in the dominant strata while the working class demonstrated class voting. Neither author tested these propositions for the late Victorian period. This analysis shows working-class consciousness functioned to unite the working class across the periphery finding its expression in class voting for the Labour-Lib-Labs, while at the same time dividing it from the enhanced ethnicity of the middle classes which found its expression in the reactive ethnicity vote for the Liberals. Ragin's untested speculations are supported. In sum, the mobilization of the periphery has two faces: a reactive ethnicity vote for the Liberals and a working-class vote for Labour-Lib-Labs. Reactive ethnicity does not appear among class-conscious lower strata voters, but among middle strata religious and ethnic voters. The Liberal party took up the politics of cultural defense in its Home Rule appeals, but Labour-Lib-Lab directed its appeal to class politics (Cole, 1950, pp. 129-131; Emy, 1973, pp. 68-71; Gregory, 1968, pp. 20-21; Hanham, 1969, pp. 91-96; Pelling, 1968, pp. 100-121), which at times functioned to divide the working class (for example, see Clarke, 1971, pp. 40-45, 253-255).

The findings so far reject the null hypothesis and the developmental hypothesis for the entire polity and the ethnic competition hypothesis in the periphery; Hechter's previously untested reactive ethnicity hypothesis is not supported, whereas Ragin's similarly untested hypothesis is supported. The focus of the analysis now turns to examining the dynamics of the center where Hechter (1975, pp. 333-339), Leifer (1981, p. 39) and Wald (1983, pp. 141-161) proffer that class voting is stronger in the center than in the periphery. The results of this analysis reject both this and the reactive ethnicity hypothesis and support the ethnic competition argument. Two broadly based party groupings appear: Conservative-Liberal Unionist and Liberal-Lib-Lab, both drawing on a mix of class and religious support. The Labour moment, the clearest expression of working class vote, is weak. Socioeconomic partisan alignments are more fluid, and the class mobilization of the early elections is blurred by a countermobilization of religion in 1906 (Blumer-Thomas, 1975, pp. 169-179; Clarke, 1971, pp. 257-258; Emy, 1973, pp. 140-141; Thompson, 1967, pp. 168-169).

The models offer support for the reactive ethnicity hypothesis in the periphery and the ethnic competition hypothesis in the center. The dynamics of the period function to divide the center from the periphery: While the periphery develops a pattern of working-class solidarity for Labour and the religious cleavage divides the middle class vote between the Conservatives and the Liberals, class voting in the center is less salient for all groups, and parties are broadly based religious and class coalitions. In the periphery working class mobilization is initially high but fragmented, but the newly enfranchised are quickly incorporated into the Lib-Lab and Labour vote. In the center high initial working class mobilization is followed by demobilization and then by partisan remobilization into the Liberal and Lib-Lab vote. However, the working-class mobilization is accompanied by a middle class and religious countermobilization which diffuses partisan class alliances. Most notably the Labour party, which is arguably the best articulator of working-class interests, does not appear in the center as a significant outlet for these dynamics.

Conclusions

This analysis examined the interplay of emerging class divisions with pre-industrial cleavages in late Victorian Britain. Previous research offered conflicting or incomplete analyses but could be formulated into four hypotheses—developmental, reactive ethnicity, ethnic competition, and null hypotheses—which facilitated the examination of three processes in late Victorian British politics: the incorporation and mobilization of the working class, the persistence of pre-industrial cleavage structures, and the response of parties to mobilization. Although the analysis did not explicitly examine the behavior of elites, it implied their efforts at mobilization, countermobilization, and structuring of electoral choices is consequential. The congruence between the electoral issues emphasized by elites and the models of aggregate electoral behavior indicate this assumption is not unwarranted (Claggett et al., 1982; Zuckerman, 1975, 1982); historians' and participants' accounts of the period supply supporting evidence.

These results go considerably beyond previous research, which is severely limited by conceptual and measurement difficulties. The processes of incorporation, mobilization, and countermobilization evidence center-periphery differences, with the center showing a more rapid working-class mobilization but also a stronger middle class and religious countermobilization functioning to diffuse class voting, findings that support the ethnic competition argument. Meanwhile, the mobilization of the periphery manifests two faces: a working-class vote for Labour and a religious and ethnic vote for the Liberals dividing the periphery against the center, findings that support the reactive ethnicity argument.

Throughout Western Europe in the late nineteenth century the expansion of franchise and the mobilization of class challenged the existing cleavage structures. The resulting potential for class polarization and cross-national working class unity remained unrealized as a countermobilization of pre-industrial cleavages muted the articulation of class conflict, fragmented the new citizenry across several parties, and delayed the expression of class voting. The consequences were enormously important for dampening political expressions of class and generating the politics of cultural defense, the latter at times finding expression in countercultural and countercenter extremist movements during the interwar period. This research extends these arguments for late Victorian Britain where the clearest evidence of class is found in the periphery increasing its distinctiveness from the center and thus diminishing overall class solidarity. The mobilization of the lower strata, which might have united class against class, instead generated a countermobilization that divided the periphery against the center; the result might be termed the peripheralization of class in late Victorian Britain. Even after the further expansion of franchise and class mobilization of the interwar period, regional distinctiveness persisted as the peripheral polities divided between class and more traditional ethnic and religious loyalties, while in Central England the Conservatives expanded their appeals to "Englishness" (Ramsden, 1978, pp. 188-215). This persistence of preexisting cleavages and their influence on the expression of newer cleavages in the polity help to explain the uneven progress toward, as well as the limits of, class politics.

Appendix: The Modeling Technique

In recent years social science analysis has benefitted from the development of data analytic techniques that mark a significant advancement over most commonly used forms of regression and factor analysis as well as earlier forms of path analysis. These newer structural equation modeling algorithms generally have several common features: the specification of directed relationships among a set of variables and a series of goodness-of-fit statistics to evaluate the explanatory power of the model by comparing the researcher's specification to the data. More recent innovations incorporate the principals of factor analysis or other data reduction techniques into a class of models called latent variable path analysis (LVPA). These models formulate theoretical or latent variables (LVs) from observed or manifest variables (MVs).

The advantages over more common data analytic techniques are several. Allowing the researcher to specify relationships among the variables not only permits, but demands, a more refined theoretical statement. Theoretically justified relationships must be postulated between MVs and LVs as well as between LVs. This procedure reduces two forms of spurious explanation: mere chance and unspecified variance which appears to explain the dependent variables but to which the researcher is unattuned. In LVPA additional advantages are present. Like factor analysis or canonical correlation, these techniques allow for data reduction without which the usual regression techniques would be overwhelmed. However, unlike factor analysis or canonical correlation LVPA models permit the researcher to specify independent and dependent variables. The researcher thus obtains the advantages of both data reduction and path analysis. A further advantage is associated with LVPA techniques: the more accurate representation of the processes under investigation. Since observed variables only rarely are isomorphic to the theoretical constructs of interest to the researcher, models that include only observed variables, typically regression analysis as well as path analysis without latent variables, incorporate measurement error into the model. Therefore, conclusions about a MV model are biased and unreliable. In contrast, LVPA modeling techniques can separate error from theoretically meaningful effects (Bentler, 1980, p. 434). LVPA models have obvious advantages, particularly in cross-group comparisons where the researcher can confidently construct theoretically similar latent variables from group specific manifest variables.

A number of algorithms exist for latent variable path analysis. The first estimation algorithm used maximum likelihood (ML) estimation methods as seen in LISREL techniques (Joereskog, 1973; Joereskog & Soerbom, 1981). Other estimation techniques have been developed by Bentler and Weeks (1980), who use generalized least-squares (GLS). These techniques achieve the advantages stated above, but at the cost of rigorous theoretical and statistical assumptions. Since LISREL primarily tests the model specified by the researcher's starting values against the covariance matrix, it is parameter-oriented; however, the level of theoretical knowledge necessary to achieve consistent starting values is rarely available in social science research. Secondly, LISREL assumes both linearity and multinormality, assumptions difficult to meet in most social science data.

Latent variable path analysis with partial least squares (LVPLS) has decided theoretical and methodological advantages over more common regression analysis as well as over similar LVPA techniques. It is particularly appropriate for research problems characterized by low information and high complexity. Developed by Lohmoeller (1981), LVPLS marks an attempt to overcome some of the limitations of LISREL and other forms of latent variable path analysis while retaining its chief advantages. LVPLS achieves its advantages by flexibility in three areas: model testing when theoretical knowledge is scarce, the choice of statistical techniques befitting the researcher's assumptions on the relationships among the MVs constituting a LV, and more relaxed distributional assumptions. LVPLS relaxes the theoretical knowledge necessary by working with a smaller number of zero-intercorrelation assumptions between the residuals and the variables (Wold, 1980, p. 70). The researcher is not required to have the profound knowledge of the proposed model as would be expected in LISREL, with the consequence that LVPLS solutions are closer to the given observations. In contrast to LISREL which is parameter-oriented and seeks to reproduce the covariance matrix of the MVs, LVPLS is prediction-oriented and seeks to reconstruct the data matrix (Falk & Stuber, 1984; Lohmoeller, 1982a, pp. 1-2; Wold, 1973, p. 384; for examples of the application to a range of social science problems, see Lohmoeller, 1982b, pp. 13-14; Nonnan & Wold, 1977, pp. 42-43; Wold, 1979, pp. 50-52).

The second flexible advantage of LVPLS is the option to use principal-component factor analysis or canonical correlation to estimate the parameters between the MVs and the LVs depending on the theoretical assumptions made about the relationships among the MVs. If the researcher assumes the MVs have a common variace underlying a theoretical construct, principal-component factor analysis is used. If the MVs are a construct designed as a categorization or measurement grouping, then canonical correlation is appropriate (Lohmoeller, 1981, pp. 14-16). The third advantage of LVPLS over techniques such as LISREL lies in its distributional assumptions. Whereas LISREL assumes linearity and multinormality in the observations, LVPLS assumes only linearity.

The distinctive features of this "soft modeling" technique are summarized by Lohmoeller (1982b, p. 12): 1) "No assumptions are made about residual covariances"; 2) "The distributional assumption is relatively soft: Only linearity, no multinormality"; 3) "Only residual variances and not residual covariances are minimized"; 4) "As a result, the measured data points are reconstructed as well as possible" and the solution is closer to the data than ML techniques.

LVPLS gains from relaxing certain assumptions at little cost to precision. The differences in the parameter estimates between LISREL and LVPLS are negligible (Lohmoeller, 1982b, p. 3; McArdle, 1982). However, the researcher should view LVPLS and LISREL as complementary rather than competitive. This is particularly true because the two techniques may be used in a complementary fashion by having LVPLS estimates as starting values in an ML solution such as LISREL. The procedure has a second advantage of markedly reducing LISREL computation time.

The attractions of LVPLS to the attendant research problem should be obvious. Latent variable path analytic techniques obtain marked advantages over the usual regression analysis: The resulting models are likely to be more precise, since the techniques reduce capitalization on chance and demand a more thorough theoretical statement before data analysis. Second, the results are more accurate and reliable since measurement error is reduced by using LVs rather than only MVs common to regression and path analysis. Third, the LVPLS solution is more valid since the distributional assumptions are closer to the real data. Finally we gain the advantages of ML techniques in final analysis by testing the LVPLS models with ML methods.

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The Political Economy of Federalism

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This article introduces a theoretical framework and an econometric methodology for analyzing the increasingly important effects of the national government on the federal system. The framework is a synthesis of the dominant political and economic approaches to this issue: it attempts to capture key elements of the complex political and administrative processes that implementation research has identified in contemporary federalism, and to exploit formal models of local fiscal choice used to analyze the impact of federal grants on state and local spending and taxing. The vehicle for the synthesis is a principal-agent model which represents the federal system as a formal hierarchy extending from Congress and the president to subnational bureaucrats. An econometric analysis of two major federal grant programs in each state for the years, 1965-1979, demonstrates that 1) economic models alone cannot explain the effects of federal grants on subnational fiscal behavior; politics must be included, and 2) the political effects can be disaggregated into ideological and constituency-oriented demands made by Congress and the White House on federal grant agencies.

The single most important development in American federalism over the last quarter century has been the sharply increased interdependence of the several levels of government. In 1960 the national government provided less than 15% of the revenue in state and local budgets and imposed relatively few constraints on subnational politics or administration. By 1978, federal aid supported 26.8% of state and local expenditures and had grown from 1.4% of GNP to 3.7%, a jump just short of the total change in the federal government's share of GNP over that period (Executive Office of the President, OMB, 1984). In addition, aid was allocated by 1978 through more than 500 categorical grants—more than three times the number extant in 1960—which often imposed severe restrictions on state and local decisionmaking (ACIR, 1982). Restrictions were also imposed by federal regulatory policies in areas such as civil rights, the environment, and occupational health and safety which states were called upon to implement. Although the growth rate of federal aid has slowed since 1979 and the burden of regulation has eased somewhat during the Reagan administration, the federal system today is dramatically more interdependent and centrally influenced than the one of 25 years ago.

Political scientists have paid substantial attention to this development, producing countless case studies of the performance of federal grant programs legislated in the 1960s and 1970s and advancing a new theoretical perspective on the intergovernmental system. The federalism described in this new "implementation" research (Bardach, 1977; Pressman & Wildavsky, 1973) is different from that captured in the "cooperative federalism" models (Elazar, 1969) of the preceding generation: it entails more political conflict, greater administrative complexity, and less policy effectiveness. Contemporary federalism is not, as the traditionally sanguine view would have it, an efficient system for sharing the economic, political, and administrative responsibilities of modern government, but rather, one that, through the initiative of the national government, has become wasteful, cumbersome, and, as often as not, unsuccessful.

This is an important new perspective, for it introduces political and bureaucratic considerations into a conceptualization in which conflicts of interest and problems of hierarchy had played a rather small role. Yet the treatment of these new considerations in implementation research still leaves something to be desired. Like the models of federalism that immediately preceded it, the one offered by implementation research is impressed with the system's complexity. The new perspective sees contemporary federalism as a highly variegated, semi-structureless system, not unlike Grodzins's (1966) classic "marble cake," but more complicated and pathologically prone to unanticipated consequences. From this perspective, it is difficult to generalize about the political and

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bureaucratic factors associated with different implementation outcomes; complexity confounds parsimonious explanations and demands the introduction of special considerations that can only be gleaned from careful case study. Theoretical work adopting the implementation perspective has therefore made little or no effort to disentangle the factors that cause the performance of the federal system to vary (although cf. Mazmanian & Sabatier, 1979). In addition, empirical work has yet to exploit statistical methods that might permit systematic comparisons of alternative causal factors in a large number of cases.

The objective of this article is to demonstrate that the conditions under which the federal system produces different outcomes can be specified formally and analyzed quantitatively. To do this I move rather far afield, indeed to an entirely different field, for theoretical and methodological guidance. Economics offers a family of formal models, developed over the same period as implementation research, for analyzing positively and normatively the performance of the federal system (e.g., Oates, 1972). Among the most developed models are those of local fiscal choice (surveyed in Inman, 1979), positive theories of the demand for government expenditure that may be used to derive or estimate, among other things, the effects of federal aid on state and local behavior. They provide the foundation for this analysis. But only the foundation.

The pure economic models are inadequate because they ignore the complicated hierarchy of political and administrative factors that implementation research has taught us are so crucial. That hierarchy must somehow be incorporated into the economic models of fiscal choice. To that end I follow the logic of another area of economic theory, "the new economics of organization" (Moe, 1984), or more specifically, "principal-agent" models. The principal-agent model guides the translation of the complex politics of implementation and intergovernmental relations into a formal hierarchy of political and administrative influence and control that constrains state and local fiscal behavior. The resulting political economy framework is then tested econometrically with an analysis of the performance of two major federal grant programs in the 50 states during the years, 1965-1979.

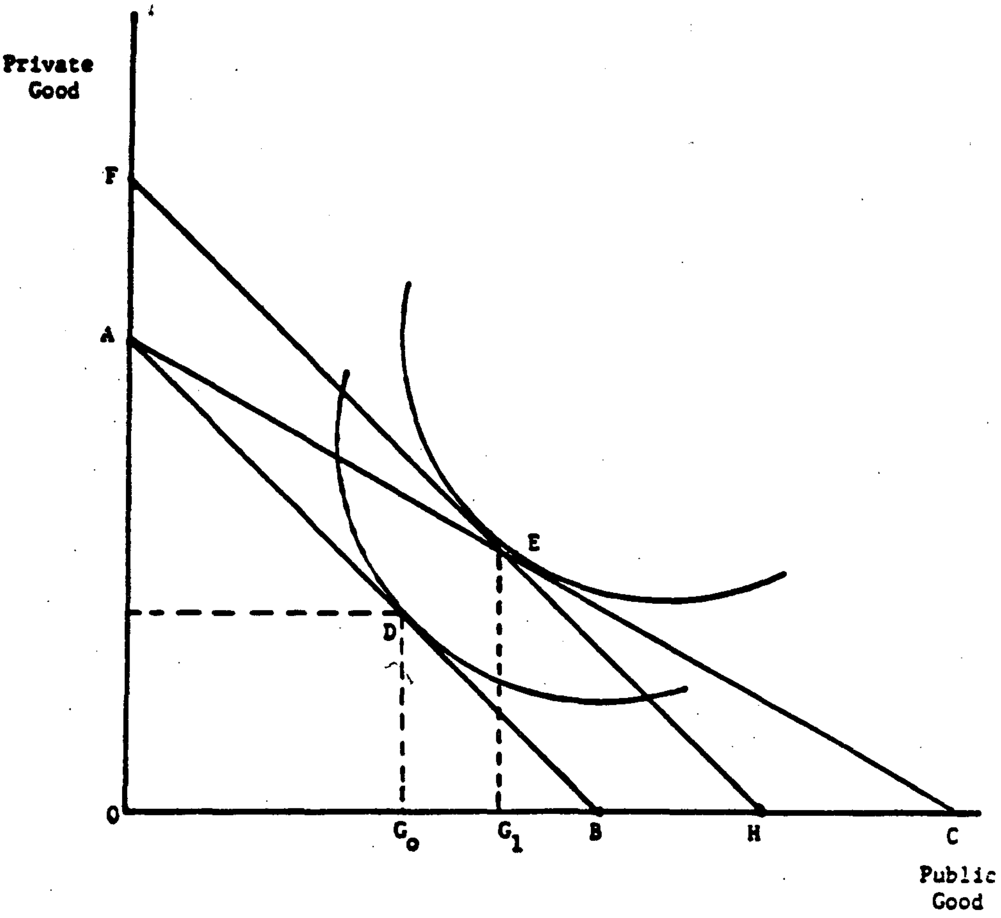
The Economics of Fiscal Federalism

Economic thinking about federalism has developed along two lines, one concerned more with normative issues, the other with positive. Although the former has become influential in political science, it is the latter, I argue, that promises greater payoffs for political analysis.

The more normative line of thought originated with Tiebout's (1956) "pure theory of local expenditure," and has focused on the existence and efficiency of equilibria under alternative arrangements for producing public goods (for a literature survey see Rose-Ackerman, 1983). The validity of Tiebout's theory—that a federal system of multiple local governments will produce public goods efficiently and overcome the obstacles identified by Samuelson (1954)—remains very much in dispute. But Tiebout's major insights—that local governments compete for taxpayers through their offerings of services and taxes, and that taxpayers demonstrate their demand for public goods through their residential location decisions—are taken seriously in economic and political analyses alike. Economics has used them to derive its venerable model of the optimal division of policy responsibilities in a federal system: a central government is the efficient location for stabilization and redistribution while multiple local governments are the more efficient arrangement for allocation; intergovernmental transfers are needed only insofar as local production generates externalities, the demand for which can only be revealed through centrally provided matching grants (Musgrave, 1959; Oates, 1973; Stigler, 1957). In Tiebout's terms, competitive local governments have strong incentives to satisfy, at minimum cost, heterogeneous tastes in nonredistributive public services, but few incentives to tax progressively, assist the poor, or responsibly manipulate fiscal or monetary instruments. Similarly, they have little economic incentive to spend intergovernmental grants on redistribution. Political science has begun to employ Tiebout's model of taxpayer mobility and government competition to explain the frequently documented reluctance of local governments to engage in redistribution unilaterally or with federal assistance (Peterson, 1981; Peterson & Wong, 1985). This analysis will test that inference by estimating the responses of state and local governments to grants for redistributive and nonredistributive purposes.

The analysis will not, however, build on the Tiebout model, but rather will follow the positive economic tradition, in which Tiebout's assumptions are of lesser importance. Whereas the normative tradition is most concerned with the performance of the federal system as a whole and takes its explanatory cue from the system's structure, the positive tradition is concerned with the decision making of local government per se. Local government competition and taxpayer mobility enter positive theories only by way of the exogenous preferences of citizens, whose aggregate demands drive local decisions, and are the subject of positive analysis.

Figure 1. Graphic Economic Model of Intergovernmental Grants



An explanation of the effects of intergovernmental grants from this perspective must be based on a model of the behavior of recipient governments, specifically on an optimizing model of local (or state) fiscal choice. Taking the local case as an example, fiscal decision making is viewed as an "as if" preference maximization subject to a budget constraint (cf. Barr & Davis, 1966; Gramlich, 1968; Henderson, 1968 for the seminal statements). Individual preferences are specified over local public services and after-tax income, given some exogenous level of need. The budget constraint requires that local government spending on public output be equal to locally raised revenue plus any external aid. Maximization yields a set of demand equations for public goods and local revenue as functions of the price of outputs, before-tax income, and public service needs.

The simplest form of such a model of intergovernmental assistance can readily be explained graphically. In Figure 1 (Bradford & Oates, 1971), the preferences of voters or citizens are already aggregated, and the indifference curves represent the (convex) preferences of a local government over a private good and a public good, the production of which an intergovernmental grant is intended to influence. The grant may take one of two general forms: a lump-sum transfer or a payment conditional on a local expenditure, that is, a match. Before the introduction of the grant, the recipient government chooses an output level of the public good, OG_0, subject to the initial budget constraint, *AB*.

If a matching grant is introduced, it has two effects on the government's original budget line: it shifts it to the right by increasing the government's income, and it makes its slope less steep by reducing the price of the public good (relative to the private good). The price reduction is determined by the matching ratio—for example, a 1:1

local:federal ratio yielding a price one-half the original or a 2:1 ratio yielding a price two-thirds the original. Graphically, a matching grant causes the budget line to pivot to the right around *A*, yielding the new budget line, *AC*. The effect of the new budget on the production of the public good is reflected in a new indifference curve tangency at *E*, and a higher level of public good output, OG_1, purchased with a matching grant, *DE*.

If the same size grant, *DE*, is lump-sum aid rather than matching assistance, the result changes. The relative price of the public and private goods is unaffected, and the only impact of the grant is on the government's income. The budget line shifts to the right, without changing slope, to a position represented by *FH*. The impact of this shift in the budget line on the production of the public good will be reflected in some quantity of output less than OG_1, given by the tangency of *GH* and the indifference curve, and because of the steeper slope of *FH* than *AB*, lying to the left of *E*. In other words, the lump-sum grant is predicted to have a less stimulating effect on public-good spending than the matching grant, because it only affects the recipient government's income, whereas the matching grant affects its income and the price of the public good.

Albeit derived from the most basic model of public choice, this conclusion is a fundamental one in the positive theory of fiscal federalism, and one that has not been substantially complicated by more sophisticated models.[1] Indeed, price and income effects are the basis for the most widely used typology of intergovernmental grants (Gramlich, 1977): open-ended matching "Type A" grants are predicted to be more stimulating of subnational spending than unconditional lump-sum "Type B" grants, with mixed "Type C" grants (conditional, closed ended, matching or lump sum) falling somewhere in between. But from a political standpoint, the economic model is inadequate. If the implementation literature has taught us anything, it is that the effect of the national government on state and local grant recipients depends heavily on the outcome of administrative and political conflicts between and within the several levels of government. The income and incentive characteristics of the grant only establish the stakes in a contest among disparate interests with different ideas about how the grant should be used. And this is but one of the major political shortcomings of the positive economic theory. In contrast to carefully modeled local production processes, local decision making is also grossly simplified. The preference function of the subnational government is taken to be the preference function of the median voter, or operationally, the family with median income. For a host of reasons (see Chubb, 1984b) this too is likely to be misleading. But the benefits of logical rigor that modeling yields seldom come without tradeoffs, and a reasonable tradeoff at this early stage of incorporating political reality is to accept some political simplification in order to specify and investigate some political complexity. Thus, I have chosen to elaborate the noneconomic mechanisms by which federal grants influence state and local fiscal choice and to leave intact the assumption of median voter rule.

It is a choice that also comes with considerable empirical support. Econometric analyses based on formal models of local fiscal choice that use the median voter assumption have improved substantially on the brute quantitative analyses that preceded them. Whereas early quantitative analyses of local expenditures yielded elasticity estimates that varied widely and unreasonably (for a summary, see Gramlich, 1969), theoretically motivated estimation has since produced sound and sensible price, income, and aid elasticities for a wide variety of public services (reviewed in Inman, 1979). Moreover, the conditions under which empirical median family incomes are equivalent to theoretical median voter preferences are now understood (Bergstrom & Goodman, 1973; Inman, 1978; although cf. Romer & Rosenthal, 1979). The objective here, then, is to elaborate the noneconomic influences of federal grants on subnational behavior, within the framework provided by the positive economic theory, and then to test quantitatively the validity of the elaborations using econometric models derived from the positive theory.

Hierarchical Control and Federalism

In essence, the economic theory of grants conceptualizes state and local governments and the citizens within them as consumers whose consumption the national government seeks to influence by manipulating their income and the prices they pay for public goods. However, given what we know about political and administrative efforts by the national government to influence state or local behavior, a more adequate conceptualization would incorporate noneconomic as well as economic methods of influence. A promising way of accomplishing this derives not from public finance and consumer theory, but from the

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[1] The positive theory of fiscal federalism has been elaborated largely through more realistic models of local public goods production, including the addition of congestable public services (Borcherding & Deacon, 1972); industrial and commercial tax bases (Ladd, 1975); endogenous tax bases (Bradford & Kelejian, 1973); and endogenous public wages (Phillips & Votey, 1975).

"new economics of organization," a relatively young field of economics devoted to understanding nonmarket production arrangements (surveyed in Moe, 1984).

From this perspective, the grantor-grantee relationship is characterized not by conditions for efficient market exchange, but by conditions for nonmarket production—that is, by organization or hierarchy. Just as firms are organized to internalize certain exchanges that are less efficiently handled by markets, grantor-grantee relationships are structured to exploit nonmarket forms of control. Economic theories of organization (e.g., Alchian & Demsetz, 1972; Jensen, 1983) do not address issues of organizational control in the public sector, but their general conceptualization of the problem fits the nonprofit context fairly well. The problem of organizational control has three elements: 1) conflict of interest between subordinates interested in their goals and a superior with goals of his own (or of the organization), 2) asymmetric information between subordinates, each of whom knows precisely how effectively he is performing, and a superior who, in the context of a large-scale production process, can observe individual performance only indirectly, and 3) shirking, or nonperformance by subordinates when they can get away with it. The response to these problems, embodied in some form in all organizations or hierarchies, includes 1) establishing monitoring of subordinates by superiors to measure individual productivity, and 2) assigning superiors marketable title to the "residual product" of subordinate production to encourage efficient monitoring and rewarding (Alchian & Demsetz, 1972). In the language of the formal models developed to analyze these relationships (Holmstrom, 1979; Ross, 1973), a "principal" uses a mixture of monitoring and compensation to maximize the productivity of his team of "agents."

Similarly, the federal system can be viewed as a two-tiered hierarchy of principal-agent relationships. At the top are national political principals, in the Congress and the White House, who establish programs to aid state or local governments in the performance of some task. Their agents are federal grant agencies, responsible for distributing aid, writing rules governing its proper use, and overseeing state and local expenditures. Federal bureaucrats are likely to have interests different from those of representatives, senators, or presidents. For example, bureaucrats may give top priority to the interests of their best organized clientele—in many cases, state or local governments. The performance of grant agencies is also difficult to measure because it hinges ultimately on the behavior of state and local grant recipients, something bureaucrats can learn about with greater ease than Congress or presidents. Yet, if the agency is not effectively monitored and controlled, it may shirk politically imposed goals and permit grants to be used for whatever purposes state and local governments desire, for example, distributional rather than redistributional purposes (Peterson, 1981; Tiebout, 1956).

A similar logic applies to the second tier of principal-agent relationships. If it is politically controlled, the federal bureaucracy seeks to effect specific uses of its grant money, but it is opposed by recipient governments who want to spend federal aid at their discretion, and who have truly the best knowledge of whether they are complying with federal rules. The federal bureaucracy finds their performance difficult to gauge because recipients are so numerous and can easily disguise their behavior in financial reports. Although direct visitation and auditing might provide very effective monitoring, they are also quite expensive. Yet to neglect direct supervision is to invite shirking, or the use of federal money for nonfederal purposes.

Viewing the federal system as a hierarchy of principal-agent relationships offers several benefits:

1. It provides a framework for analyzing empirically important noneconomic, as well as economic elements of grant implementation. More than three-fourths of all federal aid is "Type C" money (that is, closed-ended matching or lump-sum aid with conditions governing its use) typically attempting to increase state or local spending in some area of national interest. The federal government worries that its grants will be used to pay bills that would otherwise have been gladly paid with local funds, or that its matching requirements will be satisfied with local funds that would have gone to the desired function without the matching incentive. It therefore conditions most of its aid on state or local "maintenance of effort" or "non-supplantation," two of the most common grant conditions. The stock economic argument regarding these conditions (e.g., Feldstein, 1977) has long been that they are unenforceable: supplantation cannot be prevented because it is impossible to know what the local government would have spent in the absence of the grant. Economics consequently makes little effort (but cf. Courant, Gramlich, & Rubinfeld, 1979) to distinguish theoretically or to model the differences between relatively unconditional income grants such as block grants and highly restrictive aid such as categoricals. In the long run, they are both simply lump-sum aid.

But this argument ignores the success that federal agencies have had in establishing rules and reporting requirements that local governments must follow if they are to be able to prove in a court of law that they have not diverted grant funds from federally designated purposes or substituted them for existing spending (e.g., Holl, 1980). In addition, this argument ignores the decentralized support for targeted expenditures that federal agencies have been able to establish by cultivating state and local bureaucrats whose careers are tied to the maintenance of the categorical program (Hill, 1979). Although it is certainly difficult to prove what a local government's spending intentions would have been in the absence of federal assistance, federal agencies nonetheless work through informal persuasion, careful supervision, and ultimately legal rulemaking to insure that their spending has a supplementary impact. The principal-agent framework incorporates these noneconomic efforts at control.[2]

2. The framework permits the behavior of federal grant agencies to be made endogenous. Studies of implementation typically pay little attention to the political environment of the federal bureaucracy, focusing instead on the bargaining and interactions between the bureaucracy and its clientele. While informing us of the significance of these interactions for grant effects, this focus provides little systematic perspective on why grant agencies behave as they do. At least implicitly, this perspective accepts a once venerable view of bureaucracy among political scientists; that is, the bureaucracy enjoys tremendous discretion in carrying out its mission because, among other things: Congress seeks to avoid controversy and encourage interest group participation in administration (Lowi, 1979) and engages in little oversight (Ogul, 1976); presidents find the stakes too small to get involved (Cary, 1967); and the bureaucracy monopolizes information about the true costs of producing its services (Niskanan, 1971). The appropriate focus for implementation research, from this perspective, is the relationship between the bureaucracy and its clientele.

But recent theoretical work emphasizing the bargaining power of congressional committees (Miller & Moe, 1983), the potency of the mere threat of congressional intervention (Calvert & Weingast, 1984), the effectiveness of decentralized or constituent oversight (McCubbins & Schwartz, 1984), and the strength of the institutionalized presidency (Moe, 1985) provide ample reason for viewing the bureaucracy as politically constrained. In support, empirical analyses have uncovered systematic relationships over time between the behavior of administrative agencies and the presumed political goals of presidents (Beck, 1982; Moe, 1982), congressional committees (Weingast & Moran, 1983), and courts (Moe, 1985). The principal-agent framework elevates these political actors to positions of importance.

3. The principal-agent framework is accompanied by impressive analytical tools that permit the formal specification of hierarchical relationships and the rigorous derivation of positive and normative results. Enlightening applications to private sector settings include insurance (Spence & Zeckhauser, 1971), internal labor markets (Spence, 1975), and accounting (Fama & Jensen, 1983). In time these tools may become useful for public sector applications. But only in time. The public sector differs from the private sector in important respects—for example, no "residual product" or profit to be maximized and a multiplicity of political principals for most agencies—that prevent the wholesale adoption of existing analytical techniques (discussed in Moe, 1984).

The application attempted here is therefore a preliminary one, intended to demonstrate the empirical and theoretical promise of the principal-agent framework, but to stop short of proposing a deductive model. My point is to show that political as well as economic influences by the national government on state and local governments can be conceptualized within the single theoretical framework of principal-agent theory and analyzed empirically with the rigorous quantitative methods of public finance.

Two Federal Grant Programs

The empirical analysis examines two Type C federal categorical grants: Title I of the Elementary and Secondary Education Act, a lump-sum grant to poor school districts, and Vocational Education, a dollar-for-dollar matching grant to state departments of vocational education. Economic theory predicts that the vocational education grant will be the more stimulating of the two. Vocational education grants offer a price incentive as well as an income supplement, whereas Title I grants offer only an income supplement. Moreover, Title I is attempting to stimulate redistributive spending at the local level in the face of powerful mobility disincentives (Oates, 1972; Peterson, 1981; Stigler, 1957; Tiebout, 1956), whereas vocational education is promoting spending at the state level on a service not consumed strictly by its poorest families. This prediction is strengthened further by separating Vocational Education grants into two components, a large basic grant of which states can use half for any vocational purpose they choose, and a smaller grant for use only in vocational programs for the "disadvantaged." Because the basic grant is less constrained by redistribution requirements, if economic theory is correct, it should be decidedly more stimulative than Title I.

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[2] Public finance implicitly recognizes the significance of this influence: empirical analyses of the marginal impact of Type C grants on state and local spending demonstrate effects ranging inexplicably from a mere $.32 to an impressive $2.45 (Gramlich, 1977). Further, the influence has a name, "the flypaper effect," meaning that money sticks where it hits.

However, both programs have "maintenance of effort" requirements, and federal offices charged with preventing supplantation. The performance of the federal agencies in writing and enforcing accounting rules, cultivating decentralized support, and supervising subnational spending, could produce grant effects at variance with the predictions of economic theory. Indeed, the different implementation experiences of Title I and Vocational Education suggest this may be so.

Title I—known since 1982 as Chapter 1 of the Education Consolidation and Improvement Act—is the single largest federal education program, delivering approximately $3 billion annually to the nation's poorer school districts. Although recent legislative changes may lead its reputation to change, Title I has been recognized at least since the mid 1970s as a carefully monitored and effectively implemented program. The only reliable estimate of its marginal impact on local spending (Feldstein, 1977) is for 1970, but at even that early date $.72 of every aid dollar was being used for education spending. All available evidence suggests that throughout the 1970s the impact of Title I at the local level was probably increasing (Kirst & Jung, 1980). Regulations such as "supplement, not supplant," "comparability," and "excess costs" were promulgated to make more difficult the substitution of Title I money for local money (Holl, 1980). Annual on-site monitoring of each state began in 1971, and state financial audits by the Department of Health, Education and Welfare's audit agency increased from an average of only 11 annually during the 1960s to 21 annually during the period from 1971 through 1976. Moreover, although monitoring increased, supplanting allegations by the Office of Education declined sharply (National Institute of Education, 1977).

The forces contributing to this tough regulatory regime were multiple, but among the most important was Congress (Chubb, 1985). The key Title I regulations were written by Congress rather than by the bureaucracy, and in 1978 Congress based its stringent reauthorization of Title I on a draft bill written by a "beneficiary" interest group, The Lawyer's Committee for Civil Rights Under the Law, rather than on the language supplied by the Office of Education. In regular oversight hearings, throughout the 1970s, Congress clearly expressed its concern that Title I money be administered to effect supplementary spending. Although a comprehensive estimate of the impact of Title I on local spending has never before been made, the qualitative evidence is strong that Title I has been carefully monitored by Congress, and effectively implemented by the bureaucracy.

The same cannot be said for Vocational Education. From its inception in the 1917 Smith-Hughes Act until at least 1963, when it was significantly revised in the Vocational Education Act, federal aid for vocational education was administered through a generally harmonious intergovernmental bureaucracy dominated by vocational educators (Benson, 1985). For the last 20 years Congress has periodically threatened this harmony with the discord of "set-aside" requirements, reserving portions of vocational aid for groups such as the disadvantaged, the handicapped, and the bilingual, who were not being substantially served by state-funded programs. But according to beneficiary and professional interest groups, as recently as 1980, the program was run through an exclusive, cooperative alliance between the vocational bureaucracy and the American Vocational Association, the peak association of state and local vocational administrators and instructors (Chubb, 1985). The new target populations do not play a major role in implementation. What hard evidence on agency behavior is available suggests that until very recently, the federal bureaucracy made little effort to supervise state use of federal funds. On-site monitoring did not begin until 1976, and then at the rate of only 12 states per year. Financial audits did not begin until 1979, and even in 1982, noncompliance with set-aside regulations occurred in as high as 35% of the states.[3] For most of its history, then, Vocational Education has been self-implementing: matching aid is provided with the hope that its income and price effects will prove stimulating, but little additional effort is made to ensure its proper targeting.

If these differences in implementation politics and processes have consequences for state and local behavior, the estimated impacts of the grants, derived from purely economic models, will differ from the economic predictions. Title I could be much more stimulating and Vocational Education much less stimulating than their economic incentives alone would suggest. This is, in fact, what I find for the period from 1965 to 1979. In the two analyses that follow, the first employs an economic model of subnational fiscal choice to estimate the effects on local or state spending of the three federal grants, and the second develops a model with hierarchical political and administrative influences to account for the differences in the effectiveness of the grants.

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[3] For the sources of these and the preceding enforcement figures, see Chubb (1985).

The Economic Model

Although many formulations of state or local utility functions, production functions, and budget constraints are plausible (e.g., Gramlich & Rubinfeld, 1982; Inman, 1979) the reduced form demand equations (derived by maximizing public expenditures and private income subject to the revenue constraint of own taxes, outside aid, and perhaps, past savings) include the same core of exogenous variables. The system of equations used in this analysis is representative of those found in all models of local fiscal choice assuming a process of preference maximization:

$$\left\{\begin{matrix}\text{Targeted expenditures}\\ \text{Other expenditures}\end{matrix}\right\} = f_i\,\text{(Private income, targeted aid, other aid, price, needs or tastes, error)} \tag{1}$$

$$\text{Taxes} = g\,\text{(Private income, targeted aid, other aid, price, needs or tastes, error)} \tag{2}$$

$$\text{Other expenditures} + \text{targeted expenditures} = \text{taxes} + \text{retained aid} \tag{3}$$

Although these specifications are general and therefore suitable for the analysis of most types of federal aid at either the state or local level, they are easier to understand with reference to a specific case—here, Title I of the Elementary and Secondary Education Act. Title I can be expected to influence the fiscal behavior of both state and local governments: the ultimate recipients of the money are local school districts, but the money is allocated or passed-through by state governments. The focus here is on local impacts. *Targeted Expenditures* are measured as local school spending (*SCREVLK*); that is, the model is used to estimate the effects of Title I aid on total educational expenditures by local school districts.[4] Following the median voter assumptions, *Private Income* is measured by median family income (*MEDFINC*). *Targeted Aid* is given by federal Title I grants (*ESEAK*). *Other Aid* is exhausted by two variables: state education aid (*SCREVSK*), and all other federal aid, excluding Title I, received by local government (*OFAESEAK*).[5] The price of education is measured directly by average teacher salaries (*TCHPAY*), and indirectly by a regional cost of living index (*REGCOLI*)[6] that ought to reflect differences in nonsalary education costs. A scale factor, state population (*POPULATN*), is also included as a price variable; it represents the effects of economies or diseconomies of school district size on education costs (Feldstein, 1977). The need for education—or the taste for it—is measured with two indicators of the age structure: (*PCTOLD*), which gives the proportion of the population over 65 years old, and (*PCTYOUNG*), the proportion of the population age 5-17. The value placed on education is gauged by the level of private school enrollment (*PCTPRIV*). Finally, the poverty level (*PCTPOV*), as measured by the official federal definition, is controlled because Title I aid is dependent on it. Without a control for poverty, the effect of Title I aid would be indistinguishable from it. The specification is completed with a stochastic error term.

The second equation of the model, which specifies the determinants of *Other Expenditures,* is included for accounting purposes. Although it could be used to estimate the causes of local expenditures for purposes other than education, its purpose here is to complete the specification of budgeting: whatever targeted aid does not go for targeted expenditures or tax relief must, by the requirement of a balanced budget, go for other expenditures. The budget identity is given in equation (3): the sum of current expenditures must equal the sum of taxes and aid retained (i.e., not passed through) by the unit of government in question. With these two equations serving accounting functions, the only other equation of interest is the tax equation (2). It is used to estimate the effect of targeted aid on taxes (primarily local property taxes (*GNREVLK*) in the case of Title I) by controlling for the income, aid, price, and need variables expected to influence targeted expenditures. The empirical analysis involves simultaneous estimation of only the Targeted Expenditure and Tax equations; inferences regarding other expenditures may be made using the budget identity.

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[4] Although the Title I legislation aims to increase local expenditures on educationally disadvantaged children, and not necessarily school or district expenditures as a whole, the enforcement process requires evidence from districts and schools that *total* expenditure levels are maintained and that schools with entitled children expend sums comparable to nonentitled schools, in the absence of aid. In effect, compliance with Title I regulations requires evidence of increased total expenditures —the dependent variable in the targeted expenditure equation. Were data on expenditures on disadvantaged children available, they would provide a more direct test of the effect of grants. In their absence, however, district expenditures provide a valid measure of the Title I target, or at worst, one that will understate the effect of the grant.

[5] The state education aid variable includes expenditures authorized in *state* compensatory education programs, which exist in about a dozen states and are often administered in conjunction with Title I. By including these state funds with total state aid, the effect of the *federal* Title I money can be estimated without bias.

[6] A regional cost-of-living index was constructed specially for this study. It is based on the Bureau of Labor Statistics' annual autumn budget for families of four maintaining an intermediate standard of living. Indexes were computed each year for four regions—Northeast, North Central, South, and West—and Alaska. The annual budget for any region was determined by averaging the budgets for all cities within the region surveyed by the Bureau (e.g., the Northeast includes Boston, Buffalo, New York, Philadelphia, and Pittsburgh), weighting that average by the proportion of the region's population in those cities and adding to it the weighted budget for that region's nonmetropolitan areas, as reported by the BLS. The regional budgets were converted to index scores by dividing each budget by the lowest budget in the set (the South in 1965). The index ranged from 1.00 to 3.99 with a mean of 1.66 for the years 1965-1980.

Inferences about the impact of Vocational Education grants are also based on the general budget model; only the specific indicators differentiate the vocational from the Title I analysis. The indicators of the dependent variable, *Targeted Expenditures,* are total state and local spending for vocational education (*VOCEDSLK*) and total state and local spending for disadvantaged vocational education (*DISMASLK*). Combined state and local spending, rather than only local expenditures (as in Title I), must be analyzed because separate figures are not available for most years. The impacts of two components of federal vocational education spending are analyzed: basic matching grants, and matching set asides for the disadvantaged.[7] The *Targeted Aid* variable is therefore total state basic grants (*VOCEDFK*) in one system of equations, and set aside expenditures (*DISMAFK*) in the other. *Private Income* is measured by per capita personal income (*PERSINCP*) rather than by median family income because the latter is nearly collinear with the grants. New exogenous control variables include a different measure of other federal aid (*OFAVOCK*), and percent unemployment (*PCTUNEMP*), which may affect demand for vocational training. Excluded are state aid (*SCREVSK*), because this is a state level analysis, and percent private enrollment, because no a priori reason for its connection to vocational training is apparent. The right-hand side of the tax equation is modified in the same ways and uses a new dependent variable, combined state and local tax revenue (*GENRVSKL*).

Estimation and Results

Each of the estimated models contains two equations: one for targeted expenditures, the other for taxes. Although these equations are not simultaneous in variables (that is, neither left-hand side variable appears in the other equation as an explanatory variable), the equations should not be estimated separately. Because taxing and spending decisions are linked in real budgetary processes but unlinked in the equations, a correlation is likely to emerge between the disturbance terms of the two equations. Ignoring this correlation reduces the efficiency of parameter estimates. The models are therefore estimated using Zellner's (1962) three-stage generalized least-squares routine, designed to correct this problem of "seemingly unrelated regressions." Estimation problems posed by pooled cross-section times-series data are handled with a "fixed effects" specification (by region) and a detrending variable (*YEAR*), the details of which are explained in Appendix 1. The data for the analysis include observations for the 50 states in each of the 15 years from 1965 to 1979. All fiscal variables were adjusted for differing populations across states by dividing them by total public school enrollment, and all monetary variables were adjusted to real dollars by dividing them by the regional cost of living index. The sources of the data are provided in Appendix 2.

Table 1 reports the parameter estimates and standard errors for all coefficients that survived two-tailed tests of significance at the 90% confidence level. (The fixed-effect dummies were retained in all models regardless of significancce, but other nonsignificant variables were dropped from the model by successive backward eliminations.) Leaving aside the grant impacts, the parameter estimates are generally of expected sign and magnitude, comparable to estimates in similar econometric analyses (Inman, 1979).[8] For

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[7] The set-aside for the disadvantaged from the basic grant was established in Part A, section 4(c) of the 1963 Vocational Education Act, and extended in Part B, section 122(C)(1) of the 1968 Vocational Education Act, and most recently in Part A, section 109 in the 1976 Vocational Education Act. The set-aside varied from 10% (1963-1968) to 25% (1969-1976) to 20% (1977-1980) across the three authorization periods. These vocational expenditures should not be confused with the non-matching allocation authorized under Section 102(b) of the 1968 Act and Section 140 of the 1976 Act.

[8] The only variable of unexpected sign is *PCTOLD,* an anomaly that is not unprecedented (e.g., Craig & Inman, 1982).

Table 1. Unstandardized Coefficients of Demand Equations for State and/or Local Expenditures on Education, Vocational Education, and Vocational Education for the Disadvantaged (1965-1979)

Exogenous Variables		Local Education		State and Local Vocational Education		State and Local Vocational Education for Disadvantaged	
Concept	Indicator	Expenditures *SCREVLK*	Tax revenue *GNREVLK*	Expenditures *VOCEDSLK*	Tax revenue *GNREVSLK*	Expenditures *DISMASLK*	Tax revenue *GNREVSLK*
Income	*MEDFINC*	0.05 (7.90)	0.12 (9.35)	NA	NA	NA	NA
	PERSINCP	NA	NA	NS	0.48 (11.9)	0.0007 (4.27)	0.50 (11.2)
Targeted aid	*ESEAK*	1.22 (2.19)	3.20 (2.76)	NA	NA	NA	NA
	VOCEDFK	NA	NA	0.89 (2.92)	NS	NA	NA
	DISMAFK	NA	NA	NA	NA	0.45 (2.79)	NS
Other aid	*SCREVSK*	−0.67 (21.6)	−0.66 (10.1)	NA	NA	NA	NA
	OFAESEAK	0.06 (2.41)	0.23 (4.73)	NA	NA	NA	NA
	OFAVOCK	NA	NA	0.01 (3.63)	1.06 (11.1)	NA	NA
	OFADISMK	NA	NA	NA	NA	0.002 (4.87)	0.90 (8.96)
Price	*TCHPAY*	0.01 (3.33)	0.01 (1.69)	0.002 (4.26)	0.07 (4.31)	NS	0.06 (3.73)
	REGCOLI	62.5 (3.18)	224.2 (5.65)	NS	NS	NS	NS
	POPULATN	5.0E-6 (6.46)	2.0E-5 (14.4)	7.3E-7 (6.28)	1.0E-5 (3.46)	6.5E-8 (4.99)	9.3E-6 (2.75)
Need, taste	*PCTYOUNG*	15.4 (5.95)	NS	NS	NS	0.15 (2.72)	NS
	PCTOLD	20.4 (8.78)	37.2 (9.84)	1.48 (5.56)	NS	0.11 (2.75)	NS
	PCTPOV	−9.61 (7.04)	−10.1 (3.59)	−0.40 (3.23)	NS	NS	NS
	PCTUNEMP	NA	NA	1.01 (3.47)	14.2 (1.84)	0.08 (2.43)	22.7 (2.62)
	PCTPRIV	181.5 (2.32)	331.1 (2.03)	NA	NA	NA	NA
Fixed effects	*NOREAST*	61.8 (4.51)	75.1 (2.62)	−2.49 (1.47)*	205.8 (6.44)	0.19 (1.31)*	210.1 (5.84)
	NORCENT	12.7 (1.20)*	42.5 (2.02)	−5.02 (3.58)	167.9 (5.66)	−0.30 (2.06)	163.7 (5.21)
	WEST	29.2 (3.10)	138.0 (7.03)	−5.26 (3.74)	105.0 (3.82)	−0.36 (2.61)	86.5 (2.78)
	YEAR	3.9 (1.63)	−12.9 (2.57)	1.63 (9.60)	14.1 (2.65)	0.23 (0.81)*	9.0 (1.58)
	INTERCEPT	−957.4 (5.69)	−137.9 (0.51)	−130.3 (10.9)	−1647.6 (5.65)	−9.90 (3.14)	−1335.0 (3.93)
	R^2	.74	.71	.56	.66	.29	.62

Note. *t*-ratios in parentheses. NA indicates not applicable. NS indicates nonsignificant variable ($p = .10$) dropped from model.

*Nonsignificant variable ($p = .10$) retained in model. All other variables significant ($p = .10$).

example, the wealthier a state's citizens, the higher its costs of education, the larger its young population, the greater its demand for private school, and, in the case of vocational education, the higher its unemployment rate, the higher will be its per student educational spending and taxing. The models also explain most of the variance in taxing and spending. The weighted R^2 statistics for the three systems are .74 for Title I of ESEA, .59 for Vocational Education basic grants, and .48 for the Disadvantaged Set-Aside of Vocational Education.

Given the goodness of fit of the models and the plausibility of the parameter estimates, the estimated impacts of the grants can be accepted with some confidence. General *state* aid to education (*SCREVSK*) has a negative effect on local education spending. Each dollar of state aid is used by local school districts to reduce their school expenditures $.67, and to return $.66 to the taxpayers. State aid generally has few strings attached and has been found (e.g., Craig & Inman, 1982) to supplant local expenditures. But the marginal net effects of $1.00 of *federal* education aid on targeted state or local expenditures are radically different: $1.22 in the case of Title I grants, $.89 in the case of Vocational Basic Grants, and $.43 in the case of the Vocational Set-Aside.

To interpret the federal grant effects it is necessary to take account of matching ratios and administrative costs. The marginal net effect of an aid dollar (∂Targeted Expenditures/∂Targeted Aid or $\partial TE/\partial TA$) is equal to the sum of the dollar of aid, less any state or local administrative cost (a), plus any legally required matching contribution (m), plus a "supplementation" effect (S) which may be positive, negative, or zero. The supplementation effect, or the amount of locally financed spending stimulated by the grant, is therefore given by the expression:

$$S = \partial TE/\partial TA - 1 - m + a \qquad (5)$$

Assuming that the average administrative cost of educational aid is $.04 per dollar of aid (Congressional Budget Office data set, 1981) the supplementation effects of the three programs are: ESEA Title I, +$.26; Vocational Basic Grants, -$1.07; and the Vocational Disadvantaged Set-Aside, -$1.53. In other words, each dollar of Title I aid stimulated an additional $.26 of own local educational spending, whereas $.07 of each dollar of vocational aid was skimmed off for other purposes, and matching funds were provided completely from state and local funds that would have been expended for vocational education anyway. Because the vocational targeted aid variables do not have statistically significant effects (at even the .10 level) on state and local taxes, it can be inferred that federal aid is being used to supplant existing or expected state and local vocational expenditures, and in turn, to increase expenditures for other programs. The disadvantaged vocational grant fared even worse, with $.53 diverted and false matching. On the other hand, the stimulative effects of Title I grants are estimated to have been paid for by a $3.20 per dollar of aid increase in local taxes. Although this point estimate is unrealistically high, its large standard error (1.16) allows that the true effect may be as low as $1.00. Even so, the result suggests that Title I grants, or some excluded set of circumstances coterminal with this expanding equal opportunity program,[9] promoted an increase in local tax revenue used for noneducational as well as educational purposes.

The most important point about these estimates is that they demonstrate that grant performance may be radically inconsistent with economic incentives yet consistent with implementation politics. The Title I bureaucracy experienced significant congressional pressure for more vigorous regulation of grant utilization, and at least appeared to be responsive. The vocational bureaucracy did not. Despite its matching requirement, then, Vocational Education had a substantially smaller marginal impact on educational spending than Title I grants. While vocational aid supplanted established aid and was matched with previously committed funds, the Title I grants were fully channeled into local school budgets along with a small dividend in new local spending. The vast disparity in grant impacts may have other causes, but it would be unreasonable to dismiss their connections to the disparate national political conditions associated with their implementation.

The Influence of Political and Administrative Hierarchy

Federal agencies have available a number of mechanisms—for example, financial audits, field inspections, reporting requirements, and the cultivation of professional allegiances (see above)—to encourage state and local governments to use federal grants appropriately. Both qualitative evidence and the preceding econometric analysis suggest that some combination of these factors contributed to the stimulativeness of Title I, while their essential absence contributed to the ineffectiveness of Vocational Education. So, hierarchy appears to make a difference and merits inclusion in theories of local fiscal choice. But how shall its contribution be specified and estimated?

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[9] Among possibly relevant excluded variables potentially collinear with Title I aid are: 1) school finance equalization decisions by state courts or legislators that produce higher tax burdens to equalize school expenditures, and 2) federal civil rights legislation generating state compliance costs in all state and local policy fields from the mid 1960s onward. However equalization decisions were not implemented until the late 1970s, and even then, in only a minority of states. Federal civil rights legislation is the more probable specification problem, but its effects are difficult to specify.

The federal hierarchy is conceived as a two-tiered system of principal-agent relationships, the upper tier entailing the political control of a federal bureaucratic agency by elected officials and the lower tier the administrative control of state or local grant recipients by a federal agency. Simple as this structure may be in comparison to the complicated paths of influence in implementation research, it is much more complicated than any model in the economic literature on principal-agent relationships. First, it includes multiple principals in its upper tier—Congress, its members, its committees, and the president—providing a federal agency with a complicated, often conflicting, set of demands. The agency's behavior cannot be explained as a response (optimal or otherwise) to incentives and monitoring employed by a single principal. With multiple principals, the constraints on agency decision making become theoretically complex, and the appropriate specification for a formal model, an open question. Should the constraints be specified individually and with equal weights? In politically meaningful combinations such as minimum winning coalitions? Or perhaps only if they have political power, for example, the constraints imposed by committee chairs on the majority party? We cannot say a priori how agencies weigh the multiple demands made upon them.

The second complication, foreign to economic models, is the two-tiered structure of hierarchy. The political principals seek to control federal agencies only as instruments to affect the behavior of state and local governments. They are interested in no particular output of the federal grant agencies (e.g., their rules, audits, site visits) but rather their success in bringing about desired types and amounts of state and local spending. The two tiers of the hierarchy cannot, therefore, be easily separated and analyzed individually. There is no residual product of federal agency outputs that political principals seek. What they desire are certain marginal impacts of federal dollars on state and local taxing and spending. The performance political principals ultimately want to control is at the bottom of the hierarchy. The two tiers should therefore be examined jointly.

The analysis that follows handles these problems econometrically. Although this is a second-best solution to incorporating the two-tiered hierarchy in a formal model of local fiscal choice, it is an important step beyond the exclusively economic specifications used in quantitative analyses heretofore. It also provides a systematic analysis of the effects of political environments on agency performance, something that is in short supply generally, and nonexistent in the field of federalism. The explication of the model that follows is organized around the two complications posed by the federal hierarchy.

Multiple Principals

Federal agencies administering grants-in-aid exercise their greatest, and most politically significant, discretion in the enforcement of spending regulations. Because most grant money is allocated according to statutory formulas, agencies have little control over where, or in what amount, federal funds are directed. Agencies do, however, control enforcement of regulations imposed by statute and of rules promulgated by the agency to pursue its, or its superiors', interpretations of grant purposes. The importance of this control is not lost on the political actors in positions to influence agency behavior. Every president since Lyndon Johnson has had a grand federalism program and a clear position on the enforcement of federal grant regulations. Republicans have consistently favored fewer strings, less federal supervision, and the delegation of spending discretion to the state and local governments, whereas Democrats have advocated the opposite. Congress has divided similarly, with Democrats generally voting for centralization, and Republicans decentralization. Yet, notwithstanding these policy preferences, representatives may be called upon to intervene in the enforcement of grants in ways contrary to ideological preferences, and do so willingly. For example, a Democratic representative from a poor district might assist his or her state governmental lobby if it complained that the federal government was applying excessive pressure to ensure compliance with an antipoverty program. Congressional pressures, then, can cut a bureaucracy in two ways: with general signals for stronger or weaker enforcement, and with selective constituency-specific signals to ensure compliance or provide regulatory relief.

In this analysis three hypotheses regarding congressional influence and one regarding presidential are examined. The first congressional hypothesis concerns formal oversight. Congress holds oversight hearings sometimes merely to feign concern with agency behavior, but usually to investigate seriously constituent complaints about program implementation, and to embarrass agency chiefs sufficiently to gain their cooperation. The frequency of these hearings is not unrelated to public concern with federal policy. During the mid 1970s, for example, the most frequent subjects of congressional hearings were the Environmental Protection Agency and the Federal Energy Administration (Chubb, 1983). To gauge the potential influence of oversight on federal grant administration, the annual number of days of oversight hearings devoted to Title I (*ISIGHT*) and Vocational Education (*VESIGHT*) over the period from 1965 to 1979 are analyzed. More oversight is expected to yield more enforcement.

The second congressional influence examined is committee policy preference. Because Congress divides its work among congressional committees and subcommittees specialized in major governmental functions, and the members of these panels have an interest in, and influence over, the implementation of the programs under their jurisdiction that is quite large relative to that of other members of Congress, we can expect the agencies who depend on these committees for authority and funding to pay disproportionately close attention to committee preferences. As the committees become more liberal or more conservative, we might expect the agency to follow, respectively, more liberal or more conservative implementation strategies. To examine this possibility, the average annual ideology of members of the relevant oversight panels (the Senate Committee on Labor and Public Welfare, Subcommittee on Education, and Appropriations Subcommittee on Labor and HEW, and the House Committee on Education and Labor, Subcommittee on Education, and Appropriations Subcommittee on Labor and HEW) and the annual ideology of the chairmen of these committees, was calculated from the ratings of their roll-call voting behavior by Americans for Democratic Action, a liberal interest group. Each of these indicators of congressional preference is an independent variable of potential importance; however, the following analysis simplifies this set of preferences through a weighted average (each committee or subcommittee score weighted one, and each chairman score weighted three) of committee ideology, designated *CONGID*. The grant agencies are expected to relax their enforcement of maintenance-of-effort requirements on state or local governments when *CONGID* becomes more conservative, and tighten it as *CONGID* grows more liberal.

The final congressional influence is that exercised by individual senators and representatives acting as ombudsmen for their constituents. Legislators often intercede with the bureaucracy on behalf of constituents who have failed to receive benefits to which they believe they are entitled, are having problems with federal regulations, or simply have questions about federal programs (Fiorina, 1977). In connection with the administration of federal grants, legislators are likely to receive complaints both from recipient governments who feel harassed by federal monitors and from target groups who feel they are not receiving their fair share. All legislators, however, are not equally likely to be able to rectify constituent problems. Legislators sitting on relevant oversight committees are far more likely than others to receive sympathetic hearings of their complaints. The number of representatives a state has on the cognizant committees is therefore likely to affect the attention that state receives from the agency implementing a federal grant. The direction of that attention is, however, rather difficult to predict: ombudsmen can work for either tougher or weaker enforcement of spending regulations. On the assumption, though, that these policies are intended by the authorizing committees to be supplementary, state representation on relevant committees is expected to induce tougher enforcement. Here we examine the effects of state membership on the Senate Labor Committee (*NSENCOM*) and the House Education and Labor Committee (*NHRCOM*). Further, it is expected that the more liberal a state's committee members, the tougher still will be enforcement. This effect is captured by the interaction between the number of state members on the committees and the ideology (or average ideology) of those members: (*NSENCOM* * *IDSENCOM*) and (*NHRCOM* * *IDHRCOM*).

By comparison to this rather intricate conceptualization of congressional influence, presidential influence is conceptualized crudely. Presidents influence bureaucratic behavior through budgetary pressure, appointment of agency leaders, and direct commands, to mention but the major alternatives. With the appropriate data, these influences might be sorted out. But here, only the simplest hypothesis is explored: presidential turnover, and all it implies for the funding and direction of bureaucratic agencies, will change agency implementation behavior in ways consistent with presidential preferences. For grant agencies, the hypothesis implies specifically that changes from Republican to Democratic administrations will induce movement from weaker to tougher enforcement, and vice versa. This analysis examines, with dummy variables, the differences in grant administration between the Johnson presidency, the Nixon-Ford administrations (*NIXFORD*), and the Carter years (*CARTER*).

The Two-Tiered Hierarchy

Although the problem of multiple principals complicates the specification of the exogenous influences on agency behavior, the two-tiered structure of the intergovernmental hierarchy complicates the specification of the endogenous variables the principals seek to control. The appropriate econometric specification for the two-tiered hierarchy is a two equation recursive model. The first equation has federal agency behavior as endogenous and political interests and demands as exogenous, whereas the second equation makes federal agency behavior exogenous and state and local behavior endogenous. Unfortunately, this specificatiaon cannot be easily estimated: the behavior of the federal agency that political principals seek to control is extraordinarily difficult to measure. Unlike many other federal agencies that exercise their discretion by adjudicating cases, allocating money, or processing applications (and whose performances have been analyzed quantitatively), grant agencies often have their greatest impact in deciding how to enforce legislative strings. This is accomplished partly through means that can validly be counted. The location and frequency of audits and site visits fall into this category, but mostly compliance is effected through periodic rulemaking that interprets legislation and establishes reporting requirements; through informal consultation and, if necessary, threats of audits or inspections; and through the gradual establishment of professional alliances and allegiances between categorical program administrators in Washington and in state and local government. These informal methods, I have argued elsewhere (Chubb, 1984b), are logically important and substantially in evidence. The state and local administrators whose careers are tied only to federal programs can be thought of as the federal bureaucracy's solution to its problem of agency. Notwithstanding the interest that state and local governments have in allocating federal grants as they see fit, the categorical bureaucracies that have been nourished, and in some cases mandated, by Washington have career incentives to maintain the distinctiveness of federal programs locally; to resist encroachment on their funds, and the loss of previously earmarked local funds; and to keep Washington—its politicians as well as its bureaucrats—apprised of problems. The efforts of federal agencies to encourage state and local cooperation are easy to document (e.g., Chubb, 1984b; Hill, 1979), but virtually impossible to quantify. That is the methodological problem posed by the two-tiered hierarchy.

The solution proposed here is to eschew direct indicators of agency performance on the grounds they are woefully incomplete and to reduce the hierarchy to a single equation, in which federal agency performance is specified indirectly.[10] The ultimate measure of federal agency performance is really not auditing or investigations or any activity observable in Washington, but rather the compliance of grant recipients with the interests of political principals. If the federal agency is responsive to its political principals and effective in controlling grant recipients, the marginal effects of grants on state and local taxing and spending ought to vary with the demands of political principals pertaining to particular jurisdictions at particular points in time. The econometric specification for such effects is interaction. The test of the hierarchical model of grant implementation is therefore the significance of coefficients of interaction between grants and political interests or conditions. The model estimated is the system of demand equations, given in equations (1) to (3), with interaction terms introduced in the spending equation (tax interactions are not explored).[11]

To illustrate, the spending equation for the Title I model is:

$$
\begin{aligned}
&= \mathit{SCREVLK} && (5)\\
&+ B_i\,(1, \mathit{NOREAST}, \mathit{NORCENT}, \mathit{WEST}, \mathit{YEAR}) && \text{Fixed Effects}\\
&+ B_j\,(\mathit{MEDFINC}) && \text{Income}\\
&+ B_k\,(\mathit{SCREVSK}, \mathit{OFAESEAK}) && \text{Other Aid}\\
&+ B_l\,(\mathit{TCHPAY}, \mathit{REGCOLI}, \mathit{POPULATN}) && \text{Price}\\
&+ B_m\,(\mathit{PCTYOUNG}, \mathit{PCTOLD}, \mathit{PCTPOV}, \\
&\qquad \mathit{PCTPRIV}) && \text{Need, Taste}
\end{aligned}
$$

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[10] The only available direct measure of agency performance is audit frequency and location, but the Vocational Education bureaucracy engaged in no auditing whatsoever until 1979, the year in which this analysis terminates. The frequency and location of Title I audits, which began in the late 1960s, are weakly accounted for by congressional interests, and positively associated with supplantation at the local level (Chubb, 1984a), results consistent with indirect estimates of agency performance, but very incomplete.

[11] The final models reported in this analysis exclude the grant variables (*ESEAK* and *VOCEDFK*) in noninteractive form. If these variables are included, the political effects are the same, with one exception: the interaction with *CONGID* in each model is nonsignificant because the grants are highly correlated (nearly 1.0) with the interactions between the grants and *CONGID*. Separate effects cannot be estimated; only one can remain in the model. The decision to use the interaction term is based on the substantive importance of congressional ideology in qualitative analysis.

$+ B_n$ *ESEAK(ISIGHT)* Oversight Hearings
$+ B_o$ *ESEAK(CONGID)* Oversight Ideology
$+ B_p$ *ESEAK (NSENCOM, NSENCOM * IDSENCOM)* Senate Comm. Member
$+ B_q$ *ESEAK(NHRCOM, NHRCOM * IDHRCOM)* House Comm. Member
$+ B_r$ *ESEAK(NIXFORD, CARTER)* Presidency
$+ e_s$ Error

The specifications for the model of Vocational Education basic grants are analogous. (The Disadvantaged Set-Aside portion of Vocational Education is not analyzed because of its small magnitude and low political salience.)

Estimation and Results

The Title I and Vocational Education models were estimated using the same method as in the previous section. Pairs of spending and taxing equations (equations (5) and (2)) were estimated using Zellner's three-stage least-squares routine, for the 50 states in the years from 1965 to 1979. Variables that failed two-tailed tests of significance at the 90% confidence level were dropped

Table 2. Unstandardized Coefficients of Expenditure Demand Equations, with Federal Aid Conditioned by Political Constraints (1965-1979)

Exogenous Variables		Endogenous Variables			
		Local Education Expenditures (*SCREVLK*)		State and Local Vocational Expenditures (*VOCEDFK*)	
Concept	Indicator				
Income	*MEDFINC*	0.05	(8.49)	NA	
	PERSINCP	NA		NS	
Other aid	*SCREVSK*	−0.67	(21.9)	NA	
	OFAESEAK	0.04	(1.87)	NA	
	OFAVOCK	NA		0.01	(3.45)
Price	*TCHPAY*	0.01	(3.06)	0.003	(4.73)
	REGCOLI	61.0	(3.07)	NA	
	POPULATN	3.6E-06	(4.83)	1.0E-06	(6.67)
Need, Taste	*PCTYOUNG*	17.4	(6.64)	NA	
	PCTOLD	20.6	(8.94)	1.54	(5.77)
	PCTPOV	−9.36	(7.46)	−0.46	(3.72)
	PCTUNEMP	NA		1.25	(4.17)
	PCTPRIV	188.1	(2.44)	NA	
Fixed Effects	*NOREAST*	52.0	(3.74)	−1.51*	(0.85)
	NORCENT	10.4	(0.99)*	−4.80	(3.39)
	WEST	26.7	(2.87)	−4.01	(2.87)
	YEAR	6.21	(2.31)	1.53	(6.16)
	INTERCEPT	−1179.8	(5.99)	−129.7	(7.29)
Interaction of grant (*ESEAK* or *VOCEDFK*) with:					
Oversight ideology	*CONGID*	0.024	(2.18)	0.027	(3.70)
Senate committee member	*NSENCOM*	2.14	(6.40)	0.23	(1.23)*
	IDSENCOM	−0.01	(3.08)	NS	
House committee member	*NHRCOM*	NS		0.38	(1.93)
	IDHRCOM	NS		−0.01	(3.90)
President	*NIXFORD*	−0.69	(1.70)	−0.66	(1.90)
	CARTER	NS		0.61	(2.09)
	R^2	0.75		0.57	

Note. *t*-ratios in parentheses. NA indicates not applicable, NS indicates nonsignificant variable ($p = .10$) dropped from model.

*Nonsignificant variable ($p = .10$) retained in model. All other variables significant ($p = .10$).

from the model, one by one, until all coefficients were significant. Exceptions were made for the fixed effects terms (see Appendix 1), and for one political interaction term that barely failed the significance test.

The results, reported in Table 2, support the general proposition that the federal system is responsive to the interests of political principals in Washington, in a manner consistent with a two-tiered hierarchy of political control. Each of the major theoretical influences on the behavior of federal agencies is plausibly associated with variations in the effectiveness of the grants those agencies implement. Because these political constraints are analyzed in the context of economic models of local fiscal choice, it is unlikely that their significant estimated effects are the result of excluded economic considerations.

Congress appears to influence the administration of federal aid in two ways, each representing a major perspective on the behavior of Congress. First, the policy interests of the congressional oversight structure are associated with the stimulativeness of both Title I and Vocational Education grants. The congressional oversight committees, as well as the chairmen of those committees, are in strategically advantageous positions vis-à-vis Congress as a whole, and in the case of chairmen, vis-à-vis their committees too, on matters over which they have jurisdiction. Their advantage stems from agenda control (Weingast & Moran, 1983) and a host of well-known factors such as expertise, norms of reciprocity, and management of floor debate (Dodd & Schott, 1979). As a result, their preferences are the ones to which agencies can be expected to respond, to the degree that they feel constrained by Congress. The variable, *CONGID,* summarizes the committee and chairmen ideologies of the committees and subcommittees responsible for the two grants, providing an indicator of oversight committee policy interests. It is estimated to be positively associated with the effectiveness of each grant, every one point increase in oversight ideology producing 2.4 and 2.7 cent increases in the stimulativeness of Title I and Vocational Education respectively. Over the period from 1965 to 1979, *CONGID* generally increased, moving from the low 50s in the late 1960s to a peak of 70 in 1976, then falling back slightly in the late 1970s. If we think of total ideological change over the period as roughly 10 points, it is predicted to have stimulated approximately $.25 increase in the marginal effect of each $1.00 of aid. This effect is fully consistent with other evidence, cited earlier, that Congress was demanding tougher enforcement of Title I and was legislating more set-aside regulations for Vocational Education. Although a direct indicator of those demands, the frequency of oversight hearings, had no significant effect on grant administration, it is far more important that the ideological position of the committees be related than a single measure of oversight activity.

The second mechanism of apparent congressional influence is the selective influence of members of the oversight committees on behalf of their constituents. The committees that authorize these grants have been among the most liberal in Congress and have generally expected their grants to be targeted carefully. This expectation has been decidedly stronger in the case of Title I than Vocational Education, and in the Senate Labor Committee (average ADA score 1965-1979: 68) than the House Education and Labor Committee (average ADA score: 59). As the federal agencies decide how to respond to demands for more targeting, and consequently where to begin monitoring more closely, it is reasonable to expect their attention to be directed toward states represented on the committees making the demands. This, indeed, appears to happen, for in states represented on the Senate Labor Committee, the stimulativeness of Title I is $2.14 more than in states not so represented; membership on the more conservative House committee has no effect. For Vocational Education the effects are also positive, but of far smaller magnitude—a difference consistent with the generally lesser effectiveness of Vocational Education and its history of weaker congressional pressure for enforcement. Membership on the Senate Labor Committee increases the effectiveness of Vocational Education by $.23, and membership on the House Education Committee by $.38.

This, however, is not the complete story of selective state effects. States represented on the committees are apparently not treated identically by the federal bureaucracy: conservative senators (in the case of Title I) and representatives (in the case of Vocational Education) are associated with greater grant effects than liberal senators and representatives. For every point more liberal the relevant committee member, the effectiveness of the grant declines one cent. The likely reason is straightforward. Each of these oversight committees signals the bureaucracy that enforcement is important, and increasingly so as the committees become more liberal. The bureaucracy responds positively to these cues, stepping up enforcement as the preference for it increases and working especially hard to effect compliance in states strategically represented in Congress. The committees are generally satisfied with this type of agency response, but its effectiveness also brings complaints of federal intrusion, excessive paperwork, and the like from states that have been carefully monitored. When these complaints are transmitted by congressional committee members

Table 3. Estimated Annual Effects of Federal Grants on Local and/or State Expenditures, by State, Controlling for National Political Context (1965-1979)

	ESEAK	*VOCEDFK*		*ESEAK*	*VOLEDFK*
Alabama	1.53	1.45	Montana	.99	1.29
Alaska	.99	1.32	Nebraska	.99	1.42
Arizona	1.55	1.38	Nevada	1.26	1.39
Arkansas	.99	1.32	New Hampshire	1.29	1.32
California	3.00	.40	New Jersey	2.23	.67
Colorado	2.47	1.49	New Mexico	.99	1.32
Connecticut	.99	1.16	New York	2.60	.01
Delaware	.99	1.32	North Carolina	.99	1.53
Florida	.99	1.16	North Dakota	.99	1.32
Georgia	.99	1.32	Ohio	2.01	1.69
Hawaii	.99	.88	Oklahoma	1.12	1.36
Idaho	1.06	1.36	Oregon	1.56	1.22
Illinois	1.28	1.21	Pennsylvania	2.42	1.28
Indiana	.99	.90	Rhode Island	2.44	1.46
Iowa	1.47	1.37	South Carolina	.99	1.32
Kansas	.99	1.32	South Dakota	.99	1.30
Kentucky	.99	1.05	Tennessee	.99	1.32
Louisiana	.99	1.32	Texas	1.53	1.46
Maine	1.49	1.21	Utah	1.40	1.37
Maryland	1.73	1.33	Vermont	2.63	1.47
Massachusetts	2.16	1.52	Virginia	.99	1.32
Michigan	1.03	.47	Washington	.99	.98
Minnesota	1.61	1.57	West Virginia	2.53	1.55
Mississippi	.99	1.35	Wisconsin	2.20	1.47
Missouri	1.98	1.02	Wyoming	.99	1.32

to the bureaucracy, the agency must decide how to provide relief. The model indicates that relief is provided to states represented by members of the committee most supportive of the agency, namely, the liberals that constitute the committee majorities.

The final effect of political principals on agency performance emanates from the White House. Presidents have many tools for controlling bureaucrats and, in the case of federal grants, have had clear preferences how bureaucrats should behave. Democrats have supported more careful targeting of federal aid, whereas Republicans have favored the delegation of discretion to state and local governments. These preferences show up clearly in the administration of the grants. All things being equal, the effectiveness of Title I declined $.69 and Vocational Education $.66 during the Nixon and Ford years from their levels during the Johnson administration. Then, as a result of the Carter presidency, the effectiveness of Vocational Education increased $.61, while Title I remained unchanged. In short, of the four presidential coefficients estimated, three are significant and carry the correct sign. The apparent failure of the Carter administration to stimulate Title I does not mean, of course, that Title I did not grow more effective during that period—because it did—but only that other political factors account for the change.

Another way to appreciate the importance of the complex national political demands made of the federal system is to consider them together. Because these demands vary from year to year and from state to state, the performance of the federal system will also vary. In Table 3 the variations that these political demands can produce are estimated by state, averaged over the years 1965-1979. This is done by multiplying the coefficient of each political constraint (i.e., interaction term) by its average annual value, and then summing these marginal grant impacts. The differences across states are substantial, and especially so for Title I where selective state enforcement appears to have been quite pronounced. While they must be interpreted cautiously because they mask substantial variation over time, the differences illustrate plainly the substantive importance that national political conditions may have for policy implemented through the intergovernmental system.

Conclusion

The increasing penetration of state and local government by the national government is perhaps the mostly widely commented-upon fact about the contemporary federal system. Recognition of it has stimulated an impressive body of research by political scientists documenting the politicization and complication of the intergovernmental system, largely through the initiative of the national government. In addition, it has stimulated the development of formal models and econometric methods by economists for analyzing quantitatively the fiscal effects of the new national presence in state and local policymaking. What has not developed are the techniques for analyzing formally or estimating quantitatively the role of national politics and administration in the contemporary federal system. The objective of this article has been to show how that might be done.

My approach has been to integrate the general insights of policy implementation research with the methods of public finance, using economic models of fiscal choice as a foundation. The major vehicle for integrating these two approaches is principal-agent theory. It provides a plausible structure for interpreting the complex political and administrative factors that emerge from implementation and heretofore have not been viewed as disaggregatable. In suggesting that we look systematically at the interests and demands of superiors to understand the behavior of subordinates, whose interests may differ predictably from those of superiors, the principal-agent framework draws attention to simple, but potentially important, relationships between national politicians and federal agencies, and between federal agencies and state and local grant recipients. It also draws attention to monitoring, a factor in the performance of grants that has no place in the economic models. Finally, the principal-agent logic, applied to a two-tiered model of federalism, justifies an econometric model in which the effects of national politics on state and local decision making can be estimated.

If implementation research is essentially correct in its message that the complexity of contemporary federalism clouds the contributions of particular political factors and undermines generalizations, simple hierarchical influences that can be specified econometrically should not evidence systematic effects. Similarly, if economic research is right in its view that monitoring is of lesser importance than price and income stimuli, national politics and administration should not contribute much to the explanation of grant performance. Yet the econometric analyses demonstrated first, that a grant without the benefit of matching incentives, and despite local disincentives against cooperation with redistributive programs, was more stimulating than a grant that suffered more of these problems; and second, that differences in grant performance related in a straightforward fashion to variations in the interests of politicians overseeing the agencies that monitored the grants.

Viewed in the light of implementation research and public finance, these results are surprising. But considered in broader contexts, they are not. The economic literature on organization plainly recognizes the importance of monitoring, as well as incentives, in explaining the relationship between two parties to a contract. The choice by economists to model grants without including monitoring is driven, I suspect, by the desire to contain the analysis of intergovernmental assistance within the well-developed framework of optimal fiscal choice. The economic literature acknowledges the potential importance of monitoring (e.g., Gramlich, 1977); it simply has not decided how to accommodate it formally or econometrically. In contrast, although the literature on implementation acknowledges the importance of the political factors considered in this analysis, it is dubious about their systematic impact. But considerable theoretical and empirical research by political scientists (e.g., Arnold, 1979; Moe, 1982) supports the proposition, tested here, that politicians can control the behavior of federal bureaucrats. The only difference here is that the test of control is a step removed from bureaucrats in Washington. That, however, makes little logical difference. Like all federal agencies, grant agencies must satisfy political superiors and are adequately equipped to do so. They can audit, investigate, threaten, encourage, and most important (Chubb, 1984b), cultivate professional support among state and local governments to bring about the desired level of compliance. Naturally, they do not always succeed, and some of the failure may be attributable to a federal system that has become too complex to control effectively. But weak enforcement and local domination, which may be interpreted as failure, may also be what Washington wants for a particular program in a particular place and time. That, at any rate, is the interpretation that this analysis indicates should be taken seriously.

The study of contemporary federalism has developed along two lines, one economic and the other political, that fortuitously have complementary strengths and weaknesses. By bringing these two lines together it may be possible to progress substantially further, analyzing the role of politics formally and quantitatively, and the role of economics in the context of administrative and political constraints that plainly matter. The major objective of this article has been to suggest one means, derived from the theoretical literature on principal-agent problems and the empirical literature on political control of the bureaucracy, by which a political economy of federalism might be developed.

Appendix 1

Because the data in this analysis are observations pooled across time and across states, the disturbance term in each equation, which, among other things, summarizes excluded explanatory variables, is likely to consist of both time-series-related and cross-section-related disturbances. It is not likely to be random with an expected value of zero. Among systematic characteristics it may display are: cross-sectional error correlations resulting from unspecified relationships (e.g., diffusion) between neighboring states; serial correlation resulting from the exclusion of the same explanatory variable across each point in time, and disturbances with nonzero expectations owing to different true intercepts for different years and different states (e.g., New York and Mississippi may exhibit similar patterns of budgeting but at vastly different fiscal levels). Procedures have been developed for obtaining unbiased and efficient parameter estimates when these problems are present; however, no single procedure can correct all the potential problems or is optimal in all situations (on the options, see Pindyck & Rubinfeld, 1981). The best estimation strategy may well be to try all of the possible corrections to learn fully about the structure of the error term, and then use a suitable combination of procedures to obtain final estimates. Unfortunately, that solution is both expensive and of little practicality when, as in this analysis, a very large number of model specifications must be compared.

The estimation procedure used here focuses on that problem of pooling that threatens to bias the parameter estimates, that is, the possibility of different intercepts for different states and years. The correction for this problem is straightforward. Dummy variables for all but one state and all but one year—the exclusions preclude perfect multicollinearity—are added as regressors in each of the model's equations. This fixed-effects specification will not eliminate the inefficiency of serially correlated errors, but it does treat the larger problem of bias, and may, by specifying time variables, reduce serial correlation and its attendant inefficiency (Maddala, 1977). Ultimately this analysis used a modified version of the "fixed effects" specification. Instead of including a dummy variable for each state, three regional dummies are used. It is a reasonable assumption, with directly pertinent empirical support, that major fiscal differences between states can be accurately summarized by grouping states within regions, and estimating regional differences (Craig & Inman, 1982). In other words, it is sufficient to distinguish between rapidly progressing western and sunbelt states (*WEST*), economically stagnant states in the heartland (*NORTCENT*), relatively wealthy but problem-ridden states in the northeast (*NOREAST*), and comparatively poor states in the south (specified by omission of the other dummies). Further distinctions between, for example, Mississippi and Alabama or Massachusetts and Connecticut are theoretically unnecessary, and statistically costly: they consume valuable degrees of freedom. Similarly, it is assumed, with support from other research, that the effects over time are not peculiar to individual years, but rather, are likely to take the form of a trend (Craig & Inman, 1982). A single continuous variable (*YEAR*) is therefore included in each equation.

Appendix 2.

Name	Label	Source
CONGID	Weighted average of Congressional ADA scores	ADA World
DISMAFK	Vocational Education federal matching grants for disadvantaged	U.S. Dept. of HEW, National Center for Education Statistics
DISMASLK	State and local expenditures for vocational education for disadvantaged	See *DISMAFK*
ESEAK	Federal Title I ESEA categorical grants for disadvantaged	See *DISMAFK*
GNREVLK	Local tax revenue	U.S. Bureau of Census, Governmental Finances
GNREVSLK	Combined state and local tax revenue	See *GNREVLK*
MEDFINC	Median family income	U.S. Department of Commerce, Bureau of the Census
NORCENT	North central states	IL, IN, IA, KS, MI, MN, MO, NE, ND, OH, SD, WI
NOREAST	North eastern states	CT, ME, MA, NH, NJ, NY, PA, RI, VT
OFAVOCK	Other federal aid, after vocational education basic grants	See *DISMAFK*

Name	Label	Source
OFAESEAK	Other federal aid, after ESEA grants	See *DISMAFK*
PCTOLD	Percent 65 and older	See *MEDFINC*
PCTPOV	Percent below poverty line	See *MEDFINC*
PCTPRIV	Percent private school enrollment	See *DISMAFK*
PCTUNEMP	Percent unemployed	U.S. Dept. of Labor, Employment and Training Administration
PCTYOUNG	Percent 5 to 15 years old	See *MEDFINC*
PERSINCP	Per capita personal income	U.S. Department of Commerce, Bureau of Economic Analysis
POPULATN	Population	See *MEDFINC*
REGCOLI	Regional cost of living index	U.S. Dept. of Labor, Bureau of Labor Statistics
SCREVLK	Local expenditures on public education	See *DISMAFK*
SCREVSK	State expenditures on public education	See *DISMAFK*
TCHPAY	Average teachers' salaries	See *DISMAFK*
VOCEDFK	Federal expenditures on Vocational Education basic grants	See *DISMAFK*
VOCEDSLK	State and local expenditures on general vocational education	See *DISMAFK*
WEST	Western states	AK, AZ, CA, CO, HI, ID, MT, NV, NM, OR, UT, WA, WY
YEAR	Calendar year, advanced fiscal year	

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Political Parties, World Demand, and Unemployment: Domestic and International Sources of Economic Activity

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This article replicates and extends earlier work on the politics of macroeconomic policy by considering political effects on unemployment in 14 western industrial nations between 1960 and 1983. Changes of party control of government display broadly the expected effects, namely that unemployment falls under left-wing governments and rises under right-wing governments. However, the principal conclusions of this article are that partisan effects on unemployment in open economies (that is, economies heavily dependent on trade with other countries) can only be satisfactorily estimated relative to the constraint imposed by the level of world economic activity, and that in addition to politicians' strategic incentives, political institutions and economic regime constraints also determine whether partisan effects on unemployment will be sustained, transitory, or absent. With respect to the latter, on the whole no effects are found where no such effects were promised by the new government before taking office; where one-party or dominant-partner coalitions form, the effect on unemployment is transitory, whereas where broad coalitions form, it is sustained or absent. Finally, ceteris paribus, *any partisan effects are more likely where governments secure parliamentary majorities.*

Does a change of party control of government have a sustained impact on the domestic level of unemployment? A well-known article by Hibbs (1977) argues that it does, and presents supporting estimates for Britain and the United States. But doubts persist. Unemployment is high in France today; but is it high because of, in spite of, or independent of Mitterand's socialist government? Unemployment rose in Britain in 1974 and 1975 under a Labour government, but it rose everywhere at that time, owing to the first OPEC energy shock. Did it rise more or less, or faster or slower, in Britain than one might have expected? If parties' early-term policies affect unemployment, are the effects permanent? In this article I attempt to answer such questions by replicating and extending Hibbs's analysis in a comparative analysis of recent unemployment rates in many industrial democracies.

Changes of party control of government display broadly the effects described by Hibbs, namely that unemployment falls under left-wing governments and rises under right-wing governments. However, my principal arguments are:

1) Predictable partisan effects on unemployment in open economies (that is, economies heavily dependent on trade with other countries) only arise relative to constraints imposed by world levels of economic activity; and

2) In addition to politicians' strategic incentives, political institutions and economic regime constraints determine whether partisan effects on unemployment will be sustained, transitory, or absent.[1] In particular,

a) no systematic changes in unemployment should occur where no such effects were promised by a new government before taking office;

b) in party systems where one-party or dominant-partner coalitions form, the effect on unemployment should be transitory, whereas where broad coalitions form, the effect should be sustained or absent;

c) *ceteris paribus,* any effects are more likely where governments secure parliamentary majorities.

In the period of Hibbs's study (1948-1972), there were few countries other than Britain and

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[1] In the case of a sustained input, unemployment reaches a new "equilibrium" level and stays there (subject to other shocks) until party control changes again. In the case of a transitory input, unemployment moves off course and then gradually returns to its old equilibrium level.

the United States where parties alternated in control of government. Since the late 1960s, there have been many more: Australia, Austria, Belgium, Canada, Denmark, France, Germany, Ireland, the Netherlands, Norway, and Sweden, and (stretching a point) Finland and Italy have also all had periods of left and non-left government. These provide further instances in which to investigate the effects of changes in party control.

In the first part of this article I review the literature, and in the second and third parts set out the theory, data, measurement, and estimating methodology, specifying under what circumstances partisan effects can be expected and relative to what baseline they must be measured. In the fourth section the British case is discussed in detail as an illustrative example. The fifth section tests conclusions drawn from the British model, using data from 13 other western industrial democracies from 1960 to 1983. The final part draws out further implications.

Literature

Hibbs (1977) argues that the impact of unemployment varies across social classes in Britain and the United States. Blue-collar workers are more likely to become unemployed when unemployment rises and thus *ceteris paribus* more likely to favor government intervention to reduce unemployment and to support parties that promise to do so. Left-wing parties, like the Democratic and Labour parties, organize platforms to elicit this class-based support and reward their supporters by reducing unemployment while in office. Hibbs's estimates support this hypothesis.[2]

Beck (1982) questions the stability of the party impact parameter for the American unemployment series. He concludes that treatment of unemployment generally varies between administrations of each party, and in particular that the Eisenhower and Carter administrations behaved differently from other administrations of their respective parties.[3] Carter's tenure (like Labour Chancellor Denis Healey's in Britain) coincided with the rise of monetarism to prominence in macroeconomic theory. Certainly changes in the economic theory that informs government policy decisions should have a role in these models—provided analysts clearly state their prior expectations about how incumbents will act in particular cases. Even so, Hibbs's results for the American parties and unemployment are still robust (Chappell & Keech, 1984; Hibbs, 1983).

The 1970s were a period of economic shocks, mostly connected with energy prices. If unemployment rose for reasons unconnected with domestic policy, these too need to be accounted for in the model. Between 1948 and 1972 there are only three changes of party control in Britain (1951, 1964, and 1970). If these concide with exogenous shifts in directions complementary to Hibbs's hypothesis, it may erroneously appear to be confirmed.[4] Figure 1 presents the British unemployment rate and the "world" rate (a weighted average of unemployment rates in Britain's major trading partners) during the three transitions in Britain. In two cases, 1964 and 1970, there were clear trends in world unemployment. Labour took over in 1964 in a period of declining world unemployment and the Conservatives in 1970 during a sharp rise. Only the 1951 case is one of clear partisan effects, with unemployment rising after the Conservative victory in opposition to trends in the world rate.[5]

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[2] Apart from effects of partisan policy, Hibbs models the unemployment series as an autoregressive process, reflecting accumulated past shocks and seasonal factors with an allowance after late 1966 for the effects of changes in unemployment compensation. Party policy is modelled by a two-parameter first-order dynamic process. The impact parameter measures the size of the initial shock (e.g., −.094 in the British case), and the dynamic parameter is the reequilibration speed (.692). Suppose Labour comes to power. In the next quarter, unemployment is predicted to fall by .094 (approximately one-tenth of a percentage point). The partisan input continues, so the expected fall of .094 in quarterly unemployment rates persists. For the next quarter it is expected to fall by a *further* .692 * .094, or by about .16 in all. In the next quarter, the total contribution of the party term is −(.094 + .692 * .16), and so on. This continues until a new steady-state level is reached (at −.094/(1−.692), or approximately −.31, which is largely achieved after about two years. Unemployment fluctuates about this new equilibrium level until the Conservatives return to power, after which it gradually rises again exactly as it fell before.

[3] The Eisenhower result may depend on Beck's decision to exclude wars from consideration. See Alt (1985). In the increasingly monetarist 1970s, left-wing parties were often associated with policies of austerity and fiscal contraction, as in the cases of the Carter and Callaghan administrations. However, the relative positions of the two sides have probably not changed. For instance, the Danish Social Democrats were defeated in the legislature in 1982 over an austerity package, but they were defeated by a right-wing coalition that considered the package insufficiently austere.

[4] If the effects of world demand last long enough, confounding will occur when shifts in world demand are not fully incorporated into domestic unemployment before an intervention. The problem recedes as the number of changes of party control increases, although it remains in cross-sectional results like those of Schmidt (1983).

[5] This account echoes Dow's (1968) contention that the contractionary Budget of 1952 was motivated by the

Assumptions and Expectations

But this is anecdotal criticism rather than systematic analysis. My opening question, "Does a change in party control of government have a sustained impact on the level of unemployment?" leads in turn to several questions. Under what circumstances can any impacts be expected? Economic theory apart, the answer depends on assumptions about party promises, policy interventions, and institutions. If impacts do exist, when will they be sustained? It depends on party strategy and the desire for reelection on the one hand and the constraining effects of international financial regimes on the other. Finally, relative to what do such impacts exist?

If politics and external influences did not exist, how might unemployment rates vary over time? Each new time period (say, a month) produces some shock to unemployment, the effect of all that is new that month—for example, changes in relative exchange rates and prices or a sudden supply of new workers. The shock for a given month will tend to raise or lower unemployment by some amount, other things being equal. However, the effect of this shock is not confined to the month in which it occurs, but continues to reverberate for some time, for example, as factories close or expand. Assume that a shock has its greatest effect in the first period, and that its effects each subsequent month are a constant proportion of the effect the previous month. If so, and if unemployment over time is made up of such monthly shocks, each of which is independent of all others and all the effects of which decay exponentially at equal rates, then the discrete-time representation of such behavior is the familiar first-order autoregressive process (for example, see Darby & Lothian, 1983).

From some perspectives, no differences in party policies would disturb this autoregression. Politically, if competing parties offer the electorate similar policy packages, outcomes should be the same under different parties. Alternatively, although parties make different promises, they may be so constrained in making economic policy that no differences in outcomes appear (Rose, 1980). Economically, under the rational-expectations hypothesis, systematic political attempts to alter unemployment are anticipated and offset by private sector economic agents, rendering policy neutral. Nevertheless, many other macroeconomic theories permit political effects on unemployment.

In fact, modern macroeconomic controversies (see Alt & Chrystal, 1983, chap. 3) imply a hierarchy of expectations for the possible extent of political impacts on unemployment. Under strong rational expectations, no impacts are possible. Under a weaker form allowing policy surprises or learning periods, intermittent or transitory impacts are possible. Under the monetarist natural-rate hypothesis, either transitory (above or below the natural rate) or permanent (moving to the natural rate) impacts are possible. If Keynesian multiple employment equilibria exist, or supply-side policy has structural effects, then other sustained impacts are possible.[6] Significant effects on unemployment arising from changes of party control of government can constitute evidence for and against these contending claims. For instance, repeated sustained impacts at beginnings of terms preclude any form of rational expectations. Sustained interventions at various levels of unemployment favor Keynesian over monetarist views.

Openness and World Unemployment

It has long been a commonplace in international economics that when the United States sneezes, the world catches a cold. The United States is no longer quite so big in the world economy, but in a world of largely interdependent trading partners, if several sneeze, many of the rest may catch a cold. If the world's largest economies are contracting, any expansionary efforts by the government of a nation with a small open economy will be overwhelmed. In such a case, no matter how carefully the domestic process terms are modelled, the government may appear to have increased unemployment, regardless of what its

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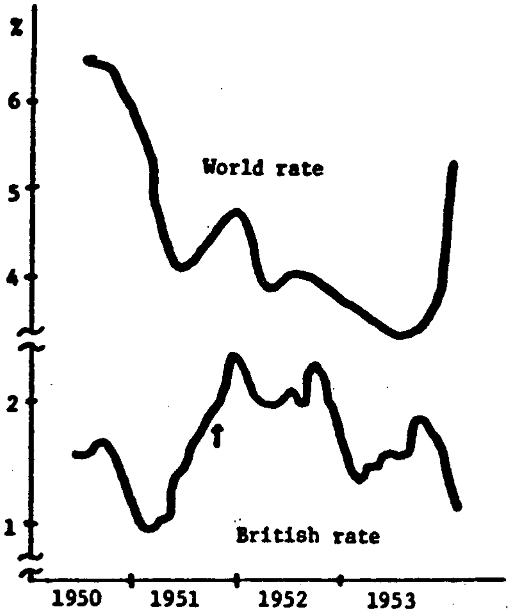
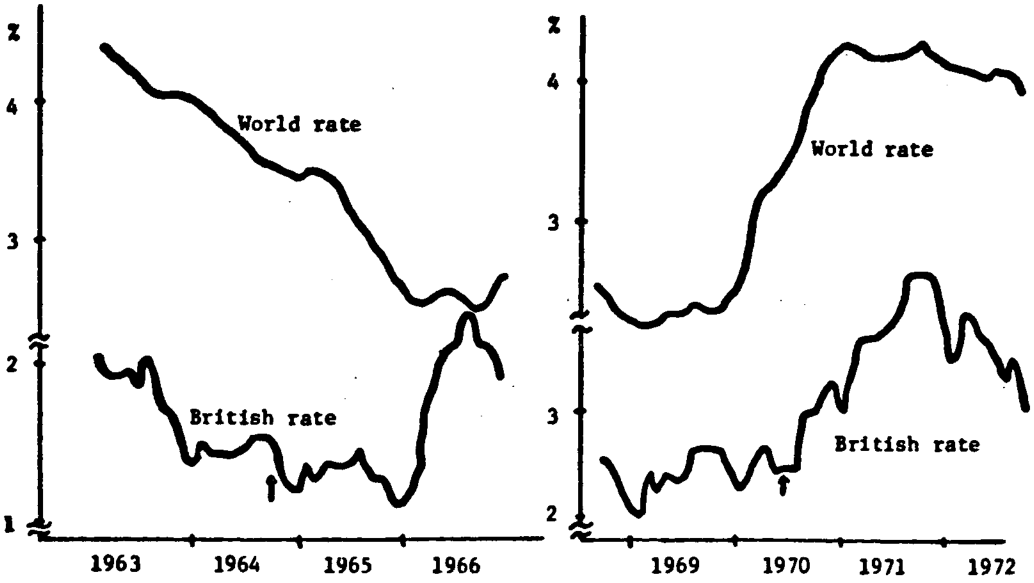
need to curb "too much demand." He also describes the coincidental Australian import quota cuts, and the subsequent return to reflation in 1953. Figure 1 also indicates that after each of the political changes, unemployment moves in the expected direction but does not reach a new equilibrium level. Rather, in each case, after two years unemployment is back where it was when the new government took over.

[6] Keynes proposed multiple aggregate employment equilibria depending on effective demand. However, monetarists argue that wage-setters' price expectations make any level of unemployment below the natural rate produce accelerating inflation, and only the natural rate itself is compatible with any level of steady inflation, which makes policy neutral in the long run. The rational expectations critique, that policy rules could be learned, anticipated, and discounted into prices, makes policy neutral in the short run. However, the Keynesian position has modern adherents who argue that government policy can affect the natural rate (for instance, by using housing subsidies to promote labor mobility) or that the long-run level of economic activity is politically determined by the structure of incentives in the tax system (Borooah & van der Ploeg, 1983).

Figure 1. British and World Unemployment Rates (Smoothed) around Elections of November 1951, October 1964, and June 1970 (Arrows indicate the month of the general election)



Election of November 1951



Election of October 1964

Election of June 1970

actual intentions were. Apart from the United States and Japan, most western industrial economies are open (dependent on trade) and the open economies are relatively small (except perhaps Germany's). They will be heavily affected by external shocks whose effects cannot be entirely offset by and which in turn considerably constrain domestic policy.

In general, if the economies of a country's major trading partners slow down, demand for that country's exports will decline (Fair, 1982). Unemployment then will increase (Nickell, 1982) unless export prices can be reduced, production shifted from present exports to other goods for which demand is higher, or employment subsidized, with the costs set against national revenues. Reducing export prices requires reducing either profits or wages (or both). Wage reduction is unlikely if strong unions are characteristic of western industrial democracies with open economies (Cameron, 1978). Profit reduction is unlikely if profits can be sustained at lower production levels or through other investments. Conversion costs hinder reallocation of production. Government subsidies or public investment will not have aggregate effects unless differences over the distributional costs of adjustment can be resolved and agreements implemented and sustained across the board, which would require a government disposed to provide subsidies and either a large state share in employment, or labor and other interest organizations centralized and sufficiently monopolistic to prevent the benefits of subsidies being frittered away through sectoral wage competition (Novotny, 1983; Olson, 1982). In the absence of these possibilities, the more open the country's economy, the larger should be the impact of world demand on domestic unemployment.

Political Effects

Apart from the constraining effects of world demand, a change of party control of government means that there is institutionally a new opportunity for some party to affect outcomes decisively, although perhaps at some political cost. Whether changes in unemployment accompany every change in party control depends on whether political effects arise automatically from class interests or instead from politicians' strategic incentives. Two assumptions are central to the argument. First, parties are organizations of politicians who have their own interests as well as ideological commitments to class-based policies (Frey, 1978). These interests and commitments can diverge. Second, reducing unemployment necessitates calculated intervention, which imposes costs on politicians, in the form either of bargaining costs to create a supporting coalition or the opportunity cost of foregone alternative policies.[7] Other things being equal, politicians will seek to reduce these costs. Under these assumptions, no change in party control should have an effect on unemployment where none was promised, and any effects which do occur can be transitory as well as sustained.

Mandates and Honeymoons. If party differences in levels of unemployment arise automatically from the interests of social classes, as Hibbs argues, then parties will attempt to create the impact on unemployment desired by their supporters, whether or not unemployment was a particularly salient issue. If strategic considerations motivate policy decisions, then effects on unemployment will appear only when parties have promised such changes and keep their promises, and not when unemployment was not a central issue. Whether unemployment, or more generally the economy, is an issue depends in turn on the strategic advantages of contending politicians (Budge & Farlie, 1983; Robertson, 1975). Consider an election campaign. Incumbents will themselves raise it as an issue only if they are happy about it, although if victorious, they have no reason to make a subsequent change in policy. If they do not raise it as an issue, their opponents may do so, if they think it electorally beneficial. If they do, and they win, they have reason to keep their promises.

In general, the need for credibility to implement future policies and the desire to have future promises believed provide political incentives to keep highly visible promises. Victorious parties initially keep promises made to core supporters as a form of investment or support-maximizing (Cox, McCubbins, & Sullivan, 1984). Because supporters and opponents alike expect election promises to be kept, there is more to be lost than gained by not keeping them. Another reason for keeping promises initially is that the so-called mandate or honeymoon of a government immediately after election makes this a particularly cheap time to pursue policies that might be more strenuously opposed at other times.[8] Only anti-system parties can oppose the victors' speedy implementation of election promises.

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[7] This article confines political effects to the impact of early-term policies on unemployment. Effects from any domestic policy made other than at the beginnings of terms are treated as part of the noise input.

[8] Keech (1982) discusses honeymoons in popular support and draws on analyses of consensus in public opinion during crises developed by Brody (1984). However, the focus on credibility in elite bargaining in this article does not require the existence of honeymoon effects in public opinion.

Two-party Systems, Coalitions, and Sustained and Transitory Effects. But why might parties, having initially kept these promises, cease to do so later on? Many answers are possible. Most simply, sustaining reduced unemployment requires sustaining intervention from period to period and repeatedly bearing the costs of such intervention. The response of unemployment to a sustained intervention (above, note 2) displays diminishing marginal returns.[9] Eventually, as the new steady state level is approached, a further period's input generates virtually no further change in unemployment. Even so, a purely programmatic party (one that values, for instance, only reduced unemployment) or a party able to secure a majority solely by maintaining reduced unemployment might sustain policy if the steady state gain from their policy exceeded the cost of maintaining the input.

For others, the choice will be different. Political capital and time are finite. If a party has a core class of support whose interests it serves, but this core class is not large enough to provide an electoral majority, when seeking reelection the government must bid for the support of other groups. Parties that seek votes across several distinct social constituencies will try different policies, some of which involve abandoning their unemployment commitment. Moreover, the agenda will change. Opposition parties will try to fight the next election on advantageous grounds. A party that reduces unemployment will be opposed by parties stressing other problems. Even parties with parliamentary majorities can find other parts of their legislative programs delayed by stubborn opposition. Thus the opportunity costs of sustaining an intervention in particular may grow very large. All these things make intervention costs increase during an incumbency. These costs will eventually exceed even the steady-state benefits of policy and will exceed the marginal benefits much sooner. If intervention costs increase during an incumbency, the incentives for transitory impacts grow.[10]

So far I have spoken of governments as though they contained one party or a dominant party united around a desired level of unemployment. If the government is a coalition of parties of which none is clearly dominant, two things will be different. First, to the extent that the party system is tied to several intersecting social cleavages, it may be unrealistic for a party to hope ever to secure a majority on its own. Its goal would then be to maximize the chance of remaining in office (in a coalition). Its objective is to secure the support of its core supporters, which dictates no change in policy and increases the probability of sustained impacts. Second, partners in a coalition can veto initiatives that might be in the electoral interest of one coalition member by threatening to break up the coalition. For instance, one party in a right-wing coalition might seek to reduce unemployment for electoral gain. If their gain disadvantaged other coalition partners, the partners would prevent the initiative. If a bargain on unemployment can be struck among coalition partners at the beginning of the incumbency, it may well be the only course of action all can agree to. Then it will be sustained throughout the incumbency (see, for example, Lijphart, 1968). Thus, among broad coalition governments, impacts on unemployment will either not exist, if no agreement can be reached, or be sustained.[11]

Majorities. Finally, regardless of whether the government is a single party or coalition, it may or may not have a parliamentary majority. If it does not, it is surely less likely to be able to implement its desired policies, because all opposition parties can unite to prevent it. If minority governments cannot claim the same mandate that produces the honeymoon for majorities, they cannot even provide transitory impacts, and certainly are unable to sustain them.

In sum, the hypotheses, which may be disconfirmed by the data, are:

1) Market mechanisms imply that the unemployment rate should exhibit autoregressive behavior (McCallum, 1978). However, much modern macroeconomic theory permits at least transitory political effects on unemployment.

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[9] The solution to the differential equation $dy/dx = bx - cy$ is the exponential decay function $y = be^{-ct}$, and the first-order autoregressive process is a discrete-time approximation of this differential equation. For a constant input, the change in unemployment with respect to time is the derivative of be^{-ct}, which (for positive b,c) is everywhere negative.

[10] The *cumulative* change in unemployment in response to a sustained input is (b/c^2) $(ct + e^{-ct} - 1)$, which is convex upward at small t and approaches a linear function at large t. The integral of a linear cost curve would be quadratic, so cumulative costs would eventually overtake benefits. Incentives for transitory interventions increase if economic constraints reduce the benefits of policy, but such constraints do not specifically predict policy reversals early in the term of office.

[11] Big sustained changes do often take place in broad coalitions (see Kohl, 1981, for examples in social spending). As with the British national government and the 1932 tariff, broad coalitions can take national-interest-related actions transcending sectoral advantages unattainable by individual parties. If credit for economic change unexpectedly goes to one party in a coalition, others might change their behavior, making the input transitory, or end the coalition, making it sustained.

Table 1. Raw and Adjusted Unemployment Rates, 4th Quarter, 1982

	Rate in Data	OECD Standard
Australia	8.4	8.6
Austria	4.5	3.8
Belgium	14.9	13.5
Canada	12.2	12.6
Denmark	10.1	n.a.
Finland	6.2	6.3
France	9.4	8.1
Germany	8.5	6.9
Ireland	17.9	n.a.
Italy	9.8	8.9
Japan	2.3	2.4
Netherlands	14.1	10.6
Norway	3.1	3.4
Sweden	2.8	3.1
United Kingdom	13.4	13.0
United States	10.3	10.5

Source. Raw data from United Nations, *Monthly Bulletin of Statistics.* Adjusted data from OECD, *Quarterly Labor Force Statistics* (Paris: OECD, 1983).

2) Political impacts on unemployment must be measured relative to world demand constraints in countries with small, open economies, in order not to confound exogenous shifts with the desires of newly incumbent politicians. The magnitude of world demand's effect corresponds to the degree of openness.

3) If outcomes result from politicians' keeping strategic promises and other early term incentives, then party changes to the left (right) should produce lower (higher) unemployment only when the economy has been an issue before the change.[12]

4) If intervention is politically costly and reelection incentives and sectoral ideologies conflict, then impacts should be transitory in systems where single parties (or one-party-dominant coalitions) form governments and sustained or absent in broad coalition governments. In either case, effects are more likely where the government has a parliamentary majority.

Methodology

Data

Unemployment. The behavior of unemployment rates was analyzed to infer the effects of party changes and world demand, using the methodology for ARIMA processes pioneered by Box and Jenkins (1976) and Box and Tiao (1975). The data for the study are the unemployment rate figures from the United Nations *Monthly Bulletin of Statistics* for the 16 countries listed in Table 1. These countries are western industrial liberal democracies in which a change of incumbency between left and right parties took place between 1960 and 1983 or that are large enough to be required for the construction of a reasonable index of world demand, or both. Data for several of these countries were collected back to January 1947 to permit replication and extension of Hibbs's work. Monthly unemployment figures were collected to provide as accurate as possible an estimation of the dynamic structure of the unemployment series.[13]

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[12] There is an asymmetry here, for although left-wing parties may promise to lower unemployment immediately, right-wing parties do not in general promise to raise it. Rather, something they promise to do involves unemployment rising later in consequence. See Alt (1985).

[13] For some periods, only quarterly figures are available for Australia, Germany, and Italy. Cross-national differences in methods of counting the unemployed are not an insuperable difficulty, because the analysis focuses on comparing time series analyzed within individual countries. Table 1 also lists the countries' unemployment rates in late 1982 with comparisons to the OECD's "standardized" rate. Apparently the distortions caused by different methods of counting are no longer as large as some years ago, but all series were checked and corrected for changes in counting the unemployed. Care was taken to avoid confounding changes in counting with changes of party control. Two episodes where there is a risk are Italy in 1962 and Belgium in 1981. In the Italian case, alternate observations back to 1960 with the new method of counting are available and have been incorporated in the data. Other countries with major discontinuities (but not near

World Demand. If a new government forms in a country with a small open economy, and the next month the world's largest economies slow down, the small country's unemployment rate will probably rise. The rise will seem to reflect the policy of the new government, because nothing in the domestic history of that country predicts a rise in unemployment at that time. To distinguish a change in activity in the world's largest economies from the policy of a new government in a country with a small open economy, the model of domestic unemployment rates must explicitly incorporate a term representing the level of economic activity in the small economy's trading environment. This relative level of economic activity is measured by a trade-weighted average of unemployment rates in nine major economies. World demand of some country *m* (with time subscripts omitted) is defined as the average of other major countries' unemployment rates *U*, weighted by the extent of exports *X* from country *m* to each of the others:[14]

$$W_m = \sum_i \left(\frac{X_i}{\sum_j X_j}\right) U_i,\ i,j \neq m.$$

World demand operates on domestic unemployment rates as does any other shock. Any month's change in world demand has its largest effects initially, and continues to have effects that decay exponentially over time. However, there will be misspecification bias if all the apparent correlation between world demand and domestic unemployment reflects only shared trends, or if (omitted) mutual feedback relations exist between each domestic economy and the world demand series. Freeman (1983) gives a careful and perceptive review of the econometric literature relating to this problem. Before incorporating world demand in the analysis of unemployment, I will investigate whether or not it appears to be causally related to domestic unemployment rates.

Party Changes and Impacts. A party designated Labour or Socialist (or the generally acknowledged more liberal of the two parties in a two-party system) will be treated as the "left" for the purpose of measuring changes of party control of government. Changes occur when the position of left parties is affected in one of the following ways: 1) replacement of one government by another with different party composition; 2) significant reallocation of ministries within an ongoing coalition, including gain or loss of the finance or economics ministry; 3) change of government support from a parliamentary minority to a minority (or vice versa) without major partisan alteration. Table 2 lists all the changes satisfying these criteria between 1960 and 1983, although a few could not be subsequently included in the analysis.[15]

When a party committed to doing something about unemployment gains an institutional position from which to carry out its proposals, it sets a new direction in policy. If it continues to move policy in this direction throughout its term, the input of party is sustained and its impact is gradual and cumulative. Party control as a sustained input is defined as dichotomous, taking one value (for example, −1) when a left-wing party is in office and another (+1) when a right-wing party is in office. (Signs can be dropped when each government's impact is estimated separately.)

Alternatively, it may push in the new direction only briefly. It starts a bureaucratic process, with some programs coming on stream right away and others later, but the push does not continue. The largest effects come first and decay over time, but the input is transitory. It is convenient to approximate the transitory input by a string of consecutive spikes (+1 or −1) at the start of each incumbency. To represent the transitory input, one could difference the sustained input (McCleary & Hay, 1979) and apply a high-order dynamic response function to represent appropriately the many bureaucratic filters through which the new policy passes while becoming more or fewer jobs. Practically, available estimation procedures do not handle the many correlated dynamic parameters of such a process well. If the input stops because of the political costs of maintaining the intervention, then the length of the input depends on the parameters of the unemployment response and cost functions (see note 10). It may, however, even stop because of international crises or con-

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changes of party control) include Britain, France, and Norway.

[14] The countries are Belgium, Britain, Canada, France, Germany, Italy, Japan, the Netherlands, and the United States. For example, world demand with respect to Belgium is average unemployment in the other eight countries, with each country's unemployment rate weighted by that country's share of Belgium's exports. Alternative measures of world demand could be based on personal consumption or GDP, but these are not available monthly.

[15] Changes of government involving succession of prime ministers of the same party (Blondel, 1968; or Dodd, 1976) need not result in changes of policy, and thus do not count here as changes of party control. Other cases were eliminated which came too close to the end of the series and where two changes in the same direction occurred within a few months. Two changes in Denmark were omitted because they produced estimation difficulties.

Table 2. Party Changes

	Direction	Economy an issue	Majority	Government
Australia				
12/72	L	N	Y	Labor single-party
11/75	R	N	Y	Liberal-Country
3/83[a]	L		Y	Labor
Austria				
3/66	R	Y	Y	Volkspartei single-party
4/70 (10/71[a])	L	N	N	Socialist minority (majority)
4/83[a]	R		Y	SP-FP coalition
Belgium				
2/66	R	N	Y	Nonsocialist CSP-PLP
6/68	L	N	Y	CSP-socialist
1/73	R	N	Y	CSP-soc-lib
4/74[a]	R		N	CSP-lib
3/77	L	N	Y	CS-soc-minor parties
12/81	R	Y	Y	Nonsocialist
Britain				
10/64	L	Y	Y	Labour
6/70	R	Y	Y	Conservative
2/74 (10/74[a])	L	Y	N	Labour (majority)
6/79	R	Y	Y	Conservative
Canada				
4/63	L	N	N	Liberal
6/68	L	N	Y	Liberal
11/72	R	N	N	Liberal
Denmark				
9/64	R	N	N	SDP loses majority
1/68	R	Y	Y	Nonsocialist
10/71	L	Y	N	Socialist
12/73	R	Y	N	Lib. democrat
2/75	L	Y	N	Socialist
8/78[a]	R		Y	SD-Liberal
10/79[a]	L		N	Socialist
9/82[a]	R		N	4-party right wing
Finland				
11/63	R	Y	Y	Nonsocialist
6/66	L	Y	Y	SD-led broad
3/70 (3/71[a])	R	Y	Y	Non-SD led broad (comm. out)
10/71	L	N	N	SD
11/75 (9/76[a])	R	Y	Y	Center-led broad (non-Left)
5/77	L	Y	Y	SD-led 5-party
France				
5/81	L	Y	Y	Socialist
Germany				
12/66	L	Y	Y	Grand Coalition
9/69	L	N	Y	SPD-FDP coalition
12/72	R	N		FDP gains econ. min.
10/82	R	Y	Y	CDU/CSU-FDP coalition
Ireland				
3/73	L	N	Y	Fine Gael/Labour coalition
7/77	R	N	Y	Fianna Fail
6/81	L	Y	Y	FG/Labour
3/82	R	Y	N	FF
12/82[a]	L		N	FG/Labour
Italy				
11/62	L	N	Y	Opening to left

Table 2 (continued)

	Direction	Economy an issue	Majority	Government
5/63[a]	R		N	CD
11/63[a]	L		Y	Socialists added
5/68	R	N	N	CD
6/72	L	N	N	Socs added, no portfolio
3/74[a]	L		N	Socs gain M. Finance
11/74	R	N	N	Nonsocialist
3/78 (8/79)	L	N	N	Communist support (SD in)
4/83[a]	R		Y	SD out
8/83[a]	L		Y	SD prime minister
Netherlands				
4/65	L	N	Y	Labor in, others out
12/66	R	Y	Y	Labor out, others in
5/73	L	Y	Y	Labor-radicals led
5/77	R	N	Y	Right-wing coalition
9/81	L	Y	Y	Labor in, D66 gain m. econ.
5/82 (11/82[a])	R	Y	N	Labor out (right-wing coal)
Norway				
10/65	R	N	Y	Nonsocialist
10/71	L	N	N	Labor
10/72	R	N	N	Nonsocialist
10/73	L	Y	N	Labor
10/81	R	Y	N	Conservative
6/83[a]	R		Y	Right-wing coalition
Sweden				
10/76	R	N	Y	Right-wing coalition
10/82	L	Y	Y	Soc.-comm. coalition

[a] Indicates a government excluded from analysis for technical reasons (see note 15). All data were compiled from *Keesing's Contemporary Archives.*

straints. Here transitory inputs are assumed to last six months, often cited as the length of the honeymoon for new incumbents. No data searching has been done to see if this is a good, bad, or indifferent representation of a transitory input. The expected response of unemployment to such transitory and sustained inputs is sketched in Figure 2.

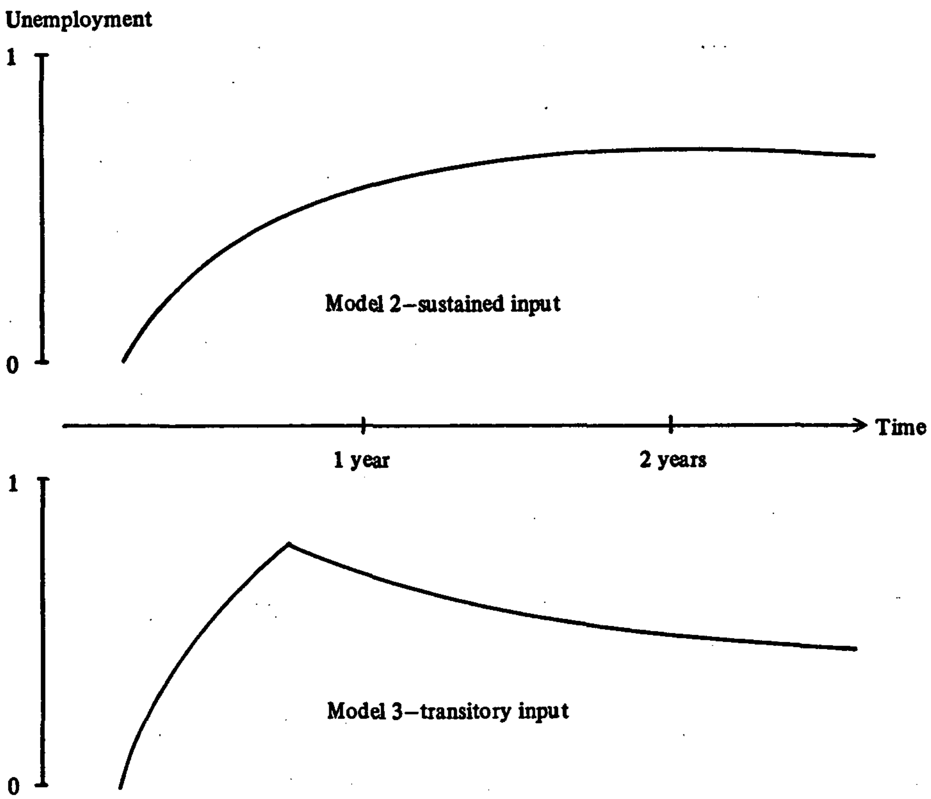
Mandates and Party Systems. If party strategies matter, then significant partisan effects on unemployment should be more common when the economy is an issue. If effects follow automatically from class interests, issue centrality or politicization will make no difference. Whether the economy was an issue was determined as follows. Each of the changes listed in Table 2 was reported in *Keesing's Contemporary Archives.* Each report contains a brief review of the major issues in the formation of the new government, whether as issues in the election (if there was one), elements in the bargaining leading to the formation of a coalition, or issues in the new government's first announcement of program in parliament. The economy was held to be an issue whenever this report specifically mentioned employment, deflation, inflation, budgetary policies like taxes or deficits, or a wage freeze. The 27 cases where the economy was an issue in this sense are indicated in Table 2. Where other issues were mentioned instead (for instance, commercial television, Catholic universities, regional parliaments, linguistic issues, foreign policy), the economy was assumed not to be central. The handful of cases where no report was available were included with the latter. The same source was used to determine the partisan allocation of cabinet portfolios, majority and minority status, and whether or not the position of left parties improved as a result of the change.

Estimation

The estimating model for any country is:

$$U_t = \frac{b_w}{1 - d_w L} W_t + \sum_k \frac{b_k}{1 - d_k L} G_{kt} + N(a_t) \qquad (1)$$

Figure 2. Response Profiles for Different Inputs



Note. The figure is obtained by simulating the model for an arbitrary start at $U = 0$, using the parameters estimated for the 1951 intervention in Britain (see Table 3, models 2 and 3). The sustained input (model 2) causes unemployment to rise in the first few quarters by .33, .49 (i.e., .33 + .48 * .33), .57, .61, .63, .64, .64. . . . The transitory input of model 3 rises by .76 in the first six months and then falls gradually back, returning to .3 after four years and more or less to zero after eight.

where U is unemployment, W is the world demand series, G is a party change variable, N denotes an ARIMA model for the error term, k is the number of changes of government, a is a white noise series of shocks, and L the lag operator. Modelling unemployment as a transfer function of world demand with intervention terms representing changes of party control is done by investigating cross-correlations to identify the transfer function, estimating the transfer function and intervention terms, and, finally, identifying and estimating the noise function (McCleary & Hay, 1979). This complex process and the extra steps taken to ascertain robustness of estimates are described in the Appendix. All models with party changes as transitory and sustained inputs are estimated separately. Final selection of the more appropriate model is based on minimum sum of squared errors.

Many unemployment series trend upward in the period, implying the need to difference the series to achieve stationarity before estimating the various parameters. Estimation without differencing a nonstationary series would result in the inclusion of many unnecessary terms. However, differencing a nearly nonstationary series may produce errors akin to those of misspecification, and inferences in such cases are problematic (Ahtola & Tiao, 1983). To meet this difficulty, a quasi-differencing operator $(1 - d_0L)$ is added to the denominator of each term of equation (1). If d_0 is estimated equal to 1, the result is identical to conventional differencing. If it equals 0, no differencing has been done. Estimation was done with a special-purpose nonlinear algorithm that also facilitated tests of restrictions on parameters. The algorithm and other details are described in the Appendix.

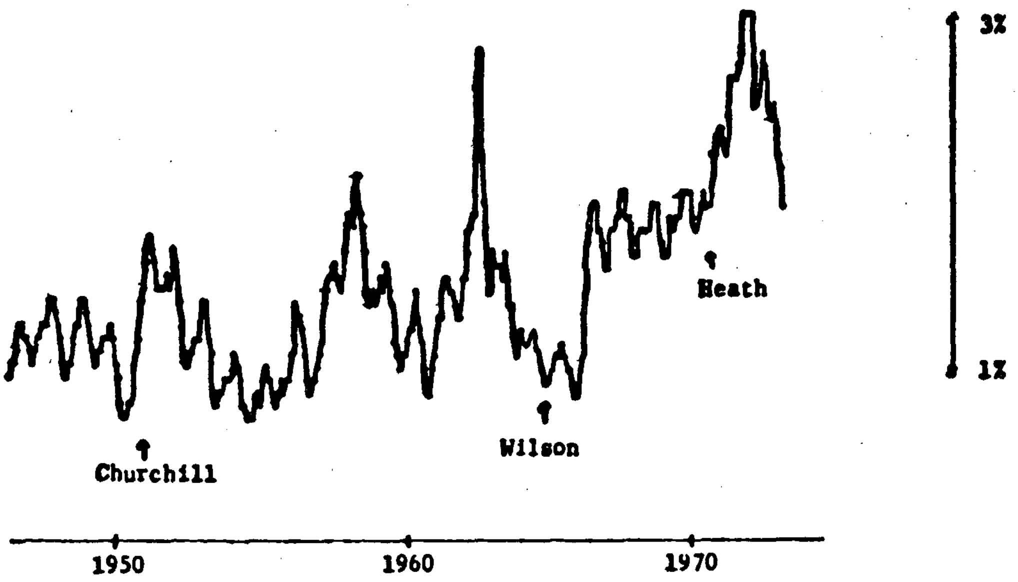
British Results

Figures 3 and 4 display the British unemployment series from 1947 to 1983 in two overlapping parts, corresponding to the estimations reported below. Periods of party incumbencies are also marked. In Figure 3 (early period), high levels of unemployment are observed both early and late, but only a very slight upward trend is discernible across the whole period. Figure 4 (later period) displays a clear upward trend throughout, broken by occasional periods of low unemployment. The next sections model party impact in the early period as transitory and investigate whether world demand is causally related to British unemployment. The last section combines transitory impacts and world demand to model the effects of party on unemployment in the more recent period. All British governments were single-party ones (minority in February 1974) and the economy was a major issue at each transition.

Early Period

Table 3 presents the estimation results for the early period. Model 0, a replication of Hibbs's published model, is reported to inspire confidence in results derived from the estimation procedures used in this article. It adds to Hibbs's model a seasonal moving-average term to compensate for overadjustment through differencing, but the main parameters, as well as the residual variance as a fraction of the overall variance, are very close to his published results.[16] The meaning of model 0 was explained above (note 2), with a change of party moving unemployment in the predicted direction by .096 of a percentage point in the initial quarter, and ultimately by .096/(1–.635) or approximately .26. Equilibrium unemployment also rises rapidly after late 1966, with the new equilibrium level 1.1 percentage points higher than the old.[17] Unemployment is otherwise a seasonal autoregressive process.

Figure 3. British Unemployment Rates, 1947-1972



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[16] Hibbs's data are the series of "wholly unemployed," whereas the present replication is based on a series including the "temporarily stopped." The latter series is noisier, but is the one usually reported in the media. Thus the replication explains less of the overall variance. Addition of the seasonal adjustment parameter has minor effects on other parameters and brings the fraction of variance explained up to that reported by Hibbs.

[17] All the results in this section include the 1966 welfare variable, although few believe that welfare policy caused most of the rise in British unemployment. Doubts rest on Labour's austere economic policies, which could also have caused the increase, as well as the timing of the onset of the increase in unemployment including the temporarily stopped, which predates the welfare policy change by a few months. (See Nickell & Andrews, 1983; or Atkinson, 1981.)

Table 3. Early Period Replications

		Model			
		0 Original	1 Transitory	2 Sustained	3 Transitory
Constant		1.6*	1.7*	1.0*	1.6*
AR(1)		.78*	.80*	.84*	.89*
MA(4) (seasonal)		.45*	.46*	.44*	.50*
Party change (-)	*b*	-.096**	-.16*		
	d	.635*	.90*		
Churchill (+) (1951)	*b*			.33*	.40*
	d			.48**	.94*
Wilson (-) (1964)	*b*			.07	-.06
	d			.93*	.12
Heath (+) (1970)	*b*			.19**	.05
	d			.76*	.98
Welfare (1966 on)	*b*	.88*	.85*	.77*	.84*
	d	.32**	.27*	.27**	.26**
Residual sum of squares		5.7	5.7	5.9	5.8
R^2		.89	.89	.89	.89

Note. Estimates are based on 100 quarterly observations for 1974-1972 in models 0 and 1 (original sum of squares = 51.3) and 104 quarterly observations for 1947-1973 in models 2 and 3 (original sum of squares = 54.0). Estimation is by nonlinear least squares. Signs on all party change dynamic coefficients (*d*) are expected to be positive.

Expected signs for partisan impact terms are in parentheses. Statistical significance is based on approximate asymptotic standard errors (Ralston & Jennrich, 1978).

*Significance at .05 level or less.
**Significant at .1 level or less.

Model 1 replaces the sustained input of model 0 with a transitory input. The impact parameter of party change nearly doubles, and the dynamic parameter indicates slower reequilibration. Because the dynamic parameter is (weakly) significantly less than one, the response is indeed transitory. Even transitory effects can be large, for the cumulative effect of party change in model 1 over a four-year period is approximately three-quarters of that estimated by Hibbs.[18] Other parameters are largely unaffected, and the fit of this model is identical to that of model 0. This identity of fit means that the British unemployment series provides no evidence by which to choose between sustained and transitory impacts.

Can a Labour government sustain reduced unemployment? Model 2 estimates sustained impacts separately for each of the early period incumbencies. (The estimation period is extended by one year, to the end of 1973, in order to have sufficient observations for the Heath administration.)[19] The results are clear. The nonpolitical parameters are stable: the *AR*(1), *MA*(4) and welfare policy adjustment values are much as in models 0 and 1. The political parameters are very different. The first shift (1951) is smaller but in the expected direction and is weakly statistically significant. The middle shift, the Labour government, now has an impact parameter with the wrong sign (implying higher unemployment under Labour), although it is statistically insignificant. This wrong sign persists even though all effects after late 1966 are ascribed to social welfare policy. Reestimating (model 3) with the separate party changes as transitory shocks produces a similar pattern of results, with marginally better fit. At least in this case the signs are all correct, even if the last two cases produce insignificant estimates. Modelling administrations separately thus also produces no evidence that (particularly left-wing) governments can sustain changes in unemployment. Moreover, the estimates of partisan impacts in models 0 and 1 are largely created by the 1951 episode, whereas the later cases appear to have been facilitated by fortuitous exogenous circumstances, which confirms the conclusions reached by inspection of Figure 1.

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[18] The 16-quarter integral of the exponential decay function for model 1 (with two unit spike for a six-month intervention) requires evaluating first the function in the second quarter (.16 + .9 * .16 = .3), then the integral for 15 quarters as though the parameters were .3 and .9, adding .16 (first quarter impact) for a total of 3.16. The 16-quarter integral of the sustained-input response (where $c = 1-d$) is $(b/c^2)\,(ct + e^{-ct} - 1)$, which evaluated at 16 and 0 for $b = .094$ and $d = .692$ gives about 4.0.

[19] In Hibbs's 1979 *APSR* communication, he claims that 1973 is well forecast by his model, and this still excludes the first OPEC shock from the analysis.

Models 2 and 3 have markedly different response profiles and estimated equilibrium levels. The major difference is that each party under model 2 produces a new equilibrium unemployment level, but model 3 leaves equilibrium unemployment where it was before the party change. (See the contrast in Figure 2, above.) The transitory-impact model in 1951 implies a total increase (gain) of unemployment of .40/(1-.94) or approximately 6.7 percentage points, spread over several years. The sustained-impact model for 1951 has a steady-state change of .33/(1-.48) or .64, but this is sustained until 1964, so the cumulative effect (in job-quarters) is much larger. But this gives the sustained-impact model peculiar counterfactual implications and unreasonable long-run properties. Under the sustained-impact model, if Labour had won the 1951 general election, unemployment in 1955 would have been 0.2% (Hibbs's estimates) or -0.5% (Model 2). If Labour had won, the transitory-impact model 3 estimates that in 1955 unemployment would have been 0.5%, against an actual value of 0.8%.

World Demand

Any attempt to estimate the party parameters of models 0 and 1 beyond 1973 produces insignificant results, and models 2 and 3 produce estimates for the next Labour government that have the wrong sign. Rather than introducing another exogenous shift (like the 1966 welfare variable) to account for the effects of the OPEC oil shock, it is better to account for the rest of the world systematically. How much does British unemployment respond to the level of economic activity in the rest of the world? Do unemployment rates share more than trends and seasonal factors? These are complex problems, best analyzed in steps. I first provide for British unemployment a noise model of the market effects discussed above, which reduces observed unemployment to a random input plus some autoregressive-moving average processes, and next show that world demand "causes" British unemployment, in the sense associated with the work of Granger and others. Therefore it is necessary to control for external shocks while testing for partisan effects. I estimate a complete model, and then repeat the analysis for other countries in *Comparative Results.*

British Unemployment. British monthly unemployment from January 1960 to May 1983 can readily be modelled in terms of its own past values and past errors. A parsimonious model which achieves this is a $(3,1,0)\times(0,1,1)_{12}$ seasonal ARIMA model. (The $(p,d,q)\times(P,D,Q)_{12}$ notation uses p for the order of autoregressive terms, q for moving average terms, and d for differences.) Because unemployment varies with months of the year in a regular way, the model includes twelfth differences and an $MA(12)$ parameter estimated at .61 to correct for annual seasonality. The $MA(12)$ parameter uses last year's error to offset partially seasonal overcompensation through differencing. The model analyzes first differences (changes in unemployment) to deal with the sustained overall increase in unemployment. Some influence of the last three changes is required to cover systematic effects. A first-order autoregressive parameter of .38 and an $AR(3)$ parameter of .24 reduce the model residuals to white noise.[20] The $AR(2)$ parameter is never significant. Thus the noise model for British unemployment contains random shocks, the three most recent first differences, plus a correction for annual seasonal effects.

World Demand and British Unemployment. To say that changes in world demand cause changes in the level of British unemployment requires that all process correlation (for instance, common trends or seasonal cycles) be treated as spurious. One series causes another in this econometric sense only if they are significantly related after all process elements have been removed from each. If the world demand series is whitened by some filter (analogous to the noise model for British unemployment) *and* if the residuals (the random input for that series) correlate significantly with the unemployment noise model residuals (the white noise input for that series) at some positive lag, then the world demand series causes British unemployment at the level of the innovations (underlying shocks) as well as the manifestations, and no spurious process correlation is involved.

World demand, a trade-weighted eight-country average of unemployment rates, can be whitened by the application of a mixed ARIMA-seasonal process model of the form $(2,1,1)\times(0,1,1)_{12}$. This model contains seasonal differencing and adjustment, plus an $MA(1)$ term of uncertain origin, and $AR(1)$ and (2) parameters whose meaning is discussed below. When its residuals are cross-

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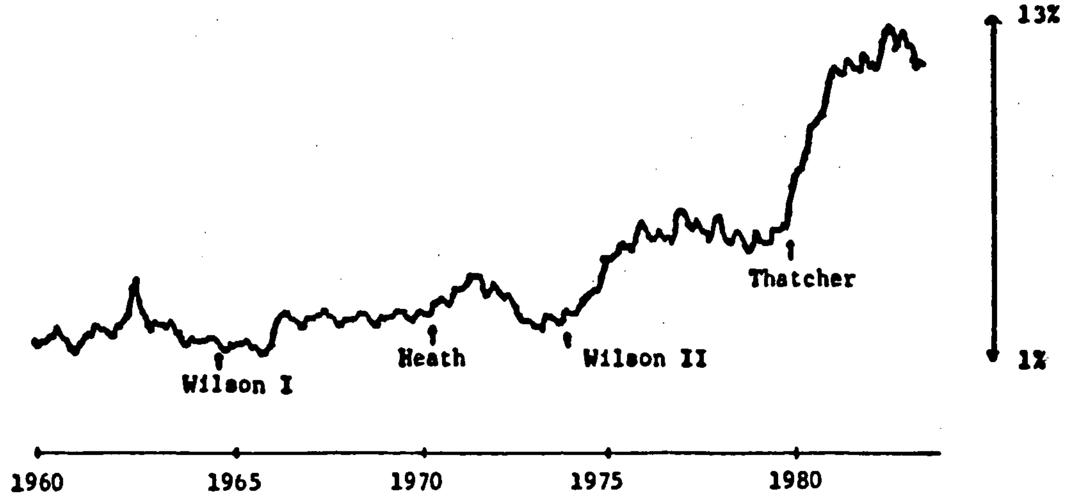
[20] The residual standard error from this model is .18. The test for white noise is the usual Box-Ljung chi-square statistic, which takes the value $x^2 = 19$ ($p = .5$) to lag 24, clearly consistent with a white noise hypothesis.

Figure 4. British Unemployment Rates, 1960-1983



correlated with the residuals from the British unemployment noise model, the result is a significant correlation at a lag of one month. A shock in world demand appears as a shock in the British unemployment series the next month.[21]

Complete Model

This section combines the underlying process moving British unemployment, the observed relationship between British and world unemployment, and the possibility of sustained and transitory party impacts. Table 4 gives the results for the period from 1960 to 1983. Models 4 and 5 extend models 2 and 3 by adding world demand. All effects are estimated simultaneously.[22] The results include ARIMA parameters for the shocks little different from those of the original noise model, with the residual standard error diminished by approximately 5% from .18 to .17. The effect of world demand is consistently and significantly estimated. Although the initial effects of a change of one percentage point in world demand are quite small, the cumulative effects ($= b/(1-d)$) are larger: from .4 (model 5) to .5 (model 4) of a percentage point of British unemployment, mostly realized ($= 3/(1-d)$) over 18 months (model 5) to three years (model 4).

To facilitate interpretation and comparison of models, the partisan impact parameter estimates

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[21] The exogeneity of world demand to British unemployment is weaker in the alternative test reported in the text below but the lag 1 cross-correlation is very clear. Note that this method provides a low-powered test, with a high probability of rejecting exogeneity when it is in fact true (Freeman, 1983). Also, if a transfer function model is fit to British unemployment with world demand as a lagged impulse input, the residuals from this model are white noise *without* application of an $AR(3)$ filter. There is evidence that shifts in world demand cause shifts in British unemployment, and that the high-order process terms in the British unemployment series represent a response to external shocks. For Britain, the model $U = -4 + 1.88W_{t-1}$ explains approximately 70% of the variance in unemployment. The residuals are white noise to any lag greater than six (probabilities vary from .1 to .3) after application of a $(1,1,0)\times(0,1,1)_{12}$ filter.

[22] Because the use of prewhitened series increases the risk of missing genuine correlations (Nelson & Schwert, 1982) and to maximize use of world demand as a control on putative political effects, observed world demand values rather than the prewhitened innovations are used in the transfer functions for all countries. Models 0 and 1 were also repeated, but when models with common party parameters across British administrations were estimated, these parameters had wrongly signed estimates. Alternatively, estimates restricting the *b* and *d* parameters to be common across administrations show that the hypothesis that all *b* parameters are equal cannot be rejected, but the hypothesis that all *d* parameters are equal can be rejected. The *d* parameters for the Right administrations are clearly larger, implying less constraint on their abilities to sustain high unemployment. However, both the Conservative governments are "short." The Heath government ended abruptly (in a period of falling unemployment), and the data period ends even before the first Thatcher term. Unfortunately, there were not enough significant estimates of dynamic parameters in transitory-impact models in the comparative analysis to test this speculation further.

Table 4. Estimates for Britain, 1960-1983

	Model	
	4 Sustained	5 Transitory
Wilson I (–) Δg or b_6	.17**	–.52**
d	.73**	.94*
Heath (+) Δg or b_6	–.08**	.66**
d	–.69	1.0*
Wilson II (–) Δg or b_6	–1.2*	–.27**
d	.79*	.87*
Thatcher (+) Δg or b_6	9.6*	1.02*
d	.96*	1.0*
World demand b	.06**	.05*
d	.88*	.83*
Constant	–.18	1.13
$AR(1)$	.37*	.38*
$MA(1)$	–.07**	–.09*
$MA(12)$	.67*	.60*
Final S.E.	.17	.17
Residual sum of squares	9.2	8.9
x^2 (prob) – lag 24	25(.4)	28(.2)

Note. Data are 282 monthly observations, January 1960-June 1983; original sum of squares = 3378. Notation follows Table 3, except that partisan impact coefficients in model 4 are changes in equilibrium unemployment levels between administrations (Δg) and in model 5 are six-month impacts (b_6).

*Significant at .05 level or less.
**Significant at .1 level or less.

are presented in a different way. For the transitory model they are the maximum impact after six months; for the sustained model they are changes in the equilibrium level of unemployment from one administration to the next. The sustained-input model 4 has the first two administrations move equilibrium unemployment in the wrong direction. The Thatcher administration clearly displays the expected effects, with equilibrium unemployment increasing by 9.6 percentage points. Including world demand demonstrates that the second Labour administration did actually reduce British unemployment, relative to world trends, by over one percentage point. The transitory-input model 5 has all administrations' impacts in the expected direction. Early-term unemployment rises under the Conservatives and falls under Labour. The effects are not quite as large in 1964 and 1970 as the 1951 episode estimates in Table 3, and are still less in 1974; but a large effect is estimated in 1979. The fit of Model 5 is similar to Model 4, and it estimates a greater number of effects consistent with prior expectations. Both models outperform the noise model.

Model 5 has a further interesting feature, apart from its good representation of the data. The dynamic parameters for the two Conservative governments are (unconstrainedly) consistent with the sustained-impact model; that is, dynamic parameters of 1.0 imply that re-equilibration never occurs. Allowing for the effects of world economic activity, it appears that under the two left-wing administrations unemployment fell, but then after a little while began to rise back toward equilibrium. Under the Conservatives unemployment rose, but its earlier equilibrium was not restored. Apart from this, however, the conclusion from examining British unemployment is that, provided world demand effects are controlled, changes of party control have the effects expected, with unemployment initially displaced by between .5 and 1 percentage point, with much larger cumulative effects in some cases.

Comparative Results

Politicians in competing parties may make promises which, if kept, affect the level of unemployment, and they have incentives to keep these promises. But politicians also face costs in keeping those promises, some of which come from constraints of an economic nature. Incumbents also have incentives to do other things in order to secure reelection. Transitory partisan impacts on British unemployment result from the natural conflict between ideological goals and the desire for reelection that governments face, as well as from structural constraints. But how generally applicable are the British results? To what extent and under what circumstances do other countries with open economies also show the pattern of transitory impacts around a steady and significant influence of world demand for the country's exports? If Britain is a special case, is it special because of the issue centrality of the economy or its single-party governments?

Available for analysis are 50 further changes of government in 13 other countries (see Table 2): six each in Finland and the Netherlands, five in Belgium, Denmark, Italy, and Norway, four in Germany and Ireland, three in Canada, two in Australia, Austria, and Sweden, and one in France. The changes are about equally split between left and right. The economy is an issue in 23 of the 50 cases (27 of 54 including Britain), and tends to appear more frequently after the oil crisis of 1973. Changes of party control of government are also more frequent in the 1970s than in the 1960s, consistent with other analyses suggesting a connection between higher levels of economic distress and more frequent episodes of government collapse (Robertson, 1983).

For each of the countries, models analogous to the British models of Table 4 were estimated. These models include a noise term, a first-order dynamic response to contemporaneous world demand, and first-order responses to party changes, defined as lasting six months in the transitory impact model and for the duration of the government in the sustained impact model. The resulting final model for each country is reported in Table 5, and is summarized term by term below. The final choice between transitory and sustained impact models was made simply on a minimum error sum-of-squares criterion. Positive coefficients for world demand are expected. Partisan impact coefficients as in Table 4.

Noise Terms

All the countries' unemployment series can be estimated as nearly differenced, first-order seasonal autoregressive-moving average processes, except as follows. Austria, Norway, and Sweden (where unemployment rates have remained low) and Italy (where it began high) do not require differencing to achieve stationarity. Australia, Germany, and Italy do not require an *MA*(1) term. All countries require seasonal adjustment with an *MA*(12) term, and where there is an *MA*(1) term, an *MA*(11) seasonal term is also required. Once estimated, all series have white noise residuals except Canada, where 8- and 9-month autocorrelations persist. In Denmark and Sweden 85 to 90% of the original variance is explained. In all other cases more than 94% is explained by the model. Finally, including the world demand term leaves the quasi-differencing parameter d_0 below 1.0 in all cases, often significantly, which implies that the persistent upward drift of unemployment in most countries is a function of the general slowing down of western economies, led by the United States after 1972.

World Demand

Significant effects of world demand are estimated in all countries except Sweden, whose unemployment rate is apparently unaffected by the world rate, even in the short run. Other countries where estimates of the impact of world demand are relatively imprecise include Denmark and Italy. However, the question remains whether world demand systematically "causes" domestic unemployment. To find out, the world demand and unemployment series for each country were whitened using conventional ARIMA methods, and the resulting noise series were cross-correlated. Positive cross-correlations between a lead and a lag of 6 months were sought, and the sum of five consecutive squared cross-correlations (see Freeman, 1983, p. 332) was subjected to a chi-square test of significance to determine exogeneity. Table 6 shows that such at least weakly significant cross-correlations in many cases (Britain, Netherlands, Ireland, Austria, Canada, and Australia) were found beginning at lag 1, in Belgium at lag 2, and in Norway, Denmark and Finland at lag 4. Lag 4 maximizes chi-square for Sweden, but the results are insignificant. No significant cross-correlations are found at less than lag 8 (two years, with quarterly data) for Italy. Both French and German unemployment are strongly correlated with unemployment in their trading environments, but at both leads and lags. Thus the countries with the largest economies, France and Germany, both affect and are affected by demand in their trading environments. Other countries with smaller open economies are directly affected by exogenous world demand, although its impact appears to be delayed or diminished in the Scandinavian countries, either by distance from the world economy, industrial or labor market structure, or domestic policy.

American Origins of World Demand. Similarly, shocks in the United States unemployment series may be shown to cause shifts in world demand. For this test, world demand is again an eight-country average, with the United States deleted, and countries are weighted by GNP. This series is whitened by application of a $(0,1,3)\times(0,1,1)_{12}$ process. The differencing and seasonal adjustment are familiar. The *MA*(3) term might represent the effect of interpolation when only quar-

Table 5. Comparative Analysis of Unemployment

Model	Australia T^{min}	Austria S^{min}	Belgium S^{min}	Canada T^{min}	Denmark S^{min}	Finland T^{min}	France T^{min}	Germany T^{min}	Ireland T^{min}	Italy S^{min}	Netherlands S^{min}	Norway T^{min}	Sweden S^{min}
g_1	.36^x	.17*	–.06^x	–.70	–1.27^x	.20	.32^x	.61*^{x}	–.02	–13.8	–.19**	–.04^x	1.11
d_1	.17	.54*	.04	.99	.28	.79**	.96*	.03	.89	.99	.29	.99	.98
g_2	–.52^x	–.21**	–.08	.05^x	.54	–1.19		–.01	.27	10.4	.20**	.01^x	–1.13
d_2	.01	.62*	.03	.01	.49	.84*		.95	.36	.19	.16	.07	.98
g_3			.09	–.55^x	–3.42*	.24*		.00	–1.13*	–4.2	–.13**	.16**	
d_3			.81**	.99	.89*	.57*		.96	.87*	.99	.21	.13	
g_4			–.06		12.69	–.31		1.79*	.46*	4.8	–.18**^{x}	–.16*	
d_4			.18		.97*	.99		.64*	.01	.99	–.24^x	.01	
g_5			.10		–6.87	.08				–6.8	–1.47*	.81	
d_5			.06		.98*	.48				.01	.86*	.99	
g_6						–.31*					2.16		
d_6						.27*					.00		
b_w	.47*	.08*	.28*	.60*	.83*	.45*	.002*	.34*	.29*	.68*	.17*	.02**	.09**
d_w	.72*	.84*	.82**	.30**	.05	.37*	.95*	.61*	.79*	.01	.82*	.16*	.16
d_0	.80*		.89*	.93*	.78*	.97*	.95*	.75*	.91*		.99*		.84*
AR(1)	.48*	.67*	–.31*	–.10*	–.33*	–.17*	.77*	.34**	.22*	.83*	.12**	.86**	–.21*
Cnst	1.86	8.55*	3.39	30.8*	1.51	15.9	–.30	.21	4.44*	4.4	–22.2	–.23	2.36*
MA(1)		.16**	.44**	.29*	.09**	.34*	.18		–.12*	–	.18*	.10**	.39*
S_{11}		.18*	.31*	.20**		.21*	.13*				.22*	.13*	.22*
S_{12}	.93	.63*	.37*	.58**	.74*	.59*	.71*	.83*	.63*	.61*	.54*	.63*	.52*
Fit	9/182	23/462	60/4531	34/1420	409/3527	24/1001	.69/62	7/457	22/2497	23/432	8/2833	7/119	12/91
$p(x^2)$	.8	.1	.6	.01	.7	.2	.1	.4	.3	.1	.9	.1	.4

Note. A model is transitory (T) or sustained (S) based on the minimum (min) error sum of squares. Partisan impact coefficients are six-month impact (transitory) or change in equilibrium level (sustained). Expected signs are negative for left changes (see Table 2) and positive for right changes. Wrong signs are superscripted *x*.

d_0: quasi-differencing parameter.
S: annual seasonal adjustment (*MA*(12) except *MA*(4) in Australia, Germany, and Italy).
Fit: ratio of residual sum of squares to original sum of squares $(1 - R^2)$.
The probability attached to the Box-Ljung chi-squared statistic is calculated to lag 12; high probabilities indicate randomness of the residuals.

*Significant at .05 level or less.
**Significant at .1 level or less.

Table 6. Economic Openness and Responses to External Shocks

Openness Rank Exports/GDP	Country	Granger test[a]		Elasticity	
		X^2	p^*	%	Rank
1	Netherlands	14.5	.05	25.8	2
2	Ireland	35.1	.01	16.0	6
3	Norway	$12.0^{(4)}$	.05	30.7	1
4	Belgium	$8.3^{(2)}$	.15	23.4	3
5	Denmark	$8.2^{(4)}$	.15	15.9	7
6	Austria	8.1	.15	20.0	5
7	Finland	$7.5^{(4)}$	.18	22.3	4
8	United Kingdom	6.8	.25	7.0	11
9	Sweden	$4.2^{(4)}$	.50	6.7	12
10	Canada	21.0	.01	13.2	10
11	Germany	$18.0^{(-2)}$	.01	15.0	8
12	Italy	5.4	.40	4.8	13
13	Australia	11.0	.05	14.8	9
14	France	$12.4^{(-2)}$	.05	1.2	14

Note. Source for openness, which is an average for 1960-1980, is OECD, *Main economic indicators.*

[a] The Granger tests relate to five correlations beginning at lag +1 unless a different lag (lead if negative) is specified by a superscript in parentheses.

[b] Probability for chi-squared with 15 degrees of freedom are .2 for $x^2 = 7$, .1 for $x^2 = 9$, .05 for $x^2 = 11$, and .01 for $x^2 = 15$.

terly data were available. The United States unemployment series is whitened by application of a $(2,1,0)\times(0,1,1)_{12}$ model. It is identical to the model suggested by Hibbs (1983). The second-order autoregressive process in United States unemployment explains the presence of similar elements in other countries' world demand series, since these include United States unemployment. When the world demand residuals are cross-correlated with the American unemployment residuals, the result is a clear first-order dynamic response from lag −1 to +2, with correlations of .23, .17, .11, and .06. Thus, shocks in the American unemployment rate *lead* rather than follow the rest-of-the-world average by a month, or, so to speak, "cause" shifts in world demand. Feedback effects from the rest of the world (the last two correlations above) are comparatively small but significant (Alt, 1985). Although this is by no means a complete model of the process, the data are consistent with any model that has shocks in American unemployment producing (at a lag, with subsequent dynamic response) shocks in world demand, which in turn produce (at a lag, or contemporaneously, with lasting response), shifts in unemployment in Britain and other countries with open economies.

Openness and World Demand. Is the size of the effect of world demand a direct function of the openness of the domestic economy? The cumulative response of domestic unemployment to a change in world demand is given by the ratio of the impact coefficient for world demand to (one minus) the dynamic coefficient. For example, in the final model in Table 5 for Belgium, the cumulative response to a unit (one percentage point) change in world demand is .28/(1−.82), 1.56 percentage points. Dividing 1.56 by Belgium's average unemployment in this period (6.17%) and multiplying by 100 converts this to an elasticity of response of domestic unemployment to world demand.[23] In Belgium, a unit change in world demand produces a cumulative change in domestic unemployment equal to 23.4% of Belgium's average unemployment in the period. These elasticities of response, based on the world demand coefficients in Table 5, are listed with corresponding ranks in Table 6.

Obvious differences exist according to openness. The seven countries with the largest proportional response to world demand are the seven countries with the most open economies. The overall correlation between ranks for openness and ranks for elasticity of response is .82, which echoes estimates of world demand effects on exports in Fair's (1982) structural model. Within this close correlation, Australia, Finland, and Norway have relatively large responses to world demand, and Sweden, the United Kingdom, and Ireland, relatively small responses. No obvious factors explicate these differences among countries. I thus conclude that, in the short term, countries with open economies are unable to offset systematically short-run domestic effects on unemployment caused by shifts in demand in their trading environments. In general, the price of openness is vulnerability to international shocks, although the possibility of some long-run differences in domestic ability to offset exogenous shocks remains, and I return to this possibility in the conclusion.

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[23] The response is 99% complete in 3/(1−.82) = 17 months, an effect long lasting enough to present serious risk of confounding (see note 4). Throughout this section, impact and dynamic parameters estimated from quarterly data (Australia, Germany, and Italy) are converted to monthly equivalents (for the transitory model) by iterative approximation of the two equations $d_q = d_m + d_m^2 + d_m^3$ and $b_m = b_q{}^*((1 + d_q)/(1 + d_q + 2d_m^4 + d_m^5))$, where q and m subscripts denote quarterly and monthly respectively.

Partisan Effects

Because two parameters, impact and dynamic, are associated with each of the changes of government, Table 5 includes estimates of 100 coefficients relating to partisan changes in the 13 countries. No significant estimates in directions consistent with expectations are achieved in Australia, Belgium, Canada, France, Italy, or Sweden. On the other hand, some partisan effects in the expected directions appear in Austria, Denmark, Finland, the Netherlands, and recently Ireland. Scattered significant coefficients appear in Belgium, Germany, and Norway. Of the total of 100 coefficients, 20 are significantly estimated with the expected sign (lower unemployment for a change to the left, higher if to the right). A further seven are weakly significant with the expected sign, four are at least weakly significant with the wrong sign, and 69 are significant. Although there is much more to say about these results, they cannot be regarded as providing sweeping confirmation for the models under discussion. Even so, there are more significant coefficients than would arise by chance. No prolonged specification searching or data mining was done to expand this proportion.

Mandates. Table 7 strongly confirms that a change in unemployment is associated with early-term policies only when parties have promised to bring one about. The 27 changes of government where the economy was *not* an issue (at least one in every country) produce 54 estimated coefficients, of which 48 are insignificant even at a .15 level, more or less what random numbers would produce. The 46 coefficients estimated when the economy was an issue produce 19 strongly significant, expected-direction effects, far more than could reasonably be believed to have occurred by chance. Four other coefficients are weakly significant, and only two are "wrong-sign" cases.[24] Significant effects in Denmark, Finland, Ireland, and the Netherlands follow salience of the economy as an issue quite closely. For instance, according to *Keesing's,* in Finland in 1963 the "first task is to balance the budget" and significant effects appear; in 1971 agricultural subsidies were the issue, and there are no significant effects on unemployment; in 1977 the "main theme" of the campaign was "unemployment" and effects appeared again.

Transitory or Sustained Model. Party systems have some effect on the permanence of impacts. Five of the 13 countries in Table 5 (Belgium, Denmark, Finland, Italy, and the Netherlands) have mainly been governed by broad coalitions in this period. The remaining eight have principally had single-party majority or minority or one-party-dominant coalition government, at least among governments of the Left.[25] Sustained impacts

Table 7. Economic Issue Centrality and Partisan Effects (%)

	Economy Was Issue		
Coefficient	Yes	No	Total
Strongly significant, right sign	19	1	20
Weakly significant, right sign	4	3	7
Insignificant	21	48	69
Weakly significant, wrong sign	0	2	2
Strongly significant, wrong sign	2	0	2
	46	54	100

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[24] Wrong signs arise in the case of the Grand Coalition in Germany, where the excluded Free Democrats and Bundesbank achieved fiscal contraction implemented by the major parties, and also in Mitterand's France.

[25] Norway and Sweden could both be modelled by one-party assumptions for the Left and coalition assumptions for the Right (see Berglund & Lindstrom, 1978), but this has not been pursued.

Table 8. Majority/Minority Governments and Partisan Effects

	Majority		
Coefficient	Two-party System	Coalition	Minority
Strongly significant, right sign	6	8	5
Weakly significant, right sign	0	3	1
Other	4	9	10

should be more characteristic of coalition governments. In fact, the sustained impact model fit better in four of the five broad coalition cases (with Finland the exception), whereas the transitory impact model fit better in six of the remaining eight countries (Austria and Sweden are the exceptions). The difference in fit between the two models in each country was rarely significant, because both contain powerful noise models. The excluded model never offered estimates more consistent (and sometimes a good deal less consistent, as in the British case) with prior expectations.

There is also limited support for the hypothesis that effects are generally less likely under coalitions, but that if they exist they will be sustained. If one takes only the parameter estimates where the economy was an issue in the 10 countries in which the "correct" model was chosen (excluding Austria, Finland, and Sweden), then 8 of 20 effect coefficients are significant under coalition governments, but 9 of 12 are significant under one-party governments (or 17 of 20 including Britain). This result is at least consistent with expectations about the impact of party strategies.[26]

Majorities and Minorities. Finally, in those cases where the economy was an issue, does it matter whether the government taking over has a parliamentary majority? The results in Table 8 are consistent with the expectation that possession of a majority, implying greater control over policy instruments, does matter. In the 23 cases of changes of party control where the economy was an issue, the government taking office was a majority one-party government in five. Six out of ten coefficients in these five cases are strongly significant and in the right direction. In a further ten cases, a broad majority coalition took over. Of the 20 resulting coefficients, 8 are strongly and 3 weakly significant and in the expected direction. In the remaining 8 cases, the government did not command a parliamentary majority. Only 5 of the 16 coefficients are strongly significant and in the expected direction. Again, the results are far from conclusive, but are clearly consistent with the argument that the possibility of decisive control of political instruments is important in achieving partisan effects on unemployment. Of course, including the British results from the fourth section with the other majority cases makes this result also appear more firmly grounded.

Conclusions

To explain the policy impact of political choice, I have integrated economic perspectives on the transmission of disturbances among trading countries with two approaches to politicians' strategic motives, one dominated by the need for reelection and the other by ideology and the representation of the preferences of social groups of supporters. Based on the recent experience of 14 western industrial countries, the evidence is clearly consistent with the proposition that a change of government in which parties of the left (right) improve their position can result in reduced (raised) levels of unemployment, provided that 1) the change in the level of unemployment is measured relative to the level of demand for a country's goods in its trading environment, and 2) no effects arise where no such effects were promised by the new government before taking office. The evidence weakly supported two other hypotheses, that in party systems where one-party or dominant-partner coalitions form, the effect on unemployment is transitory, whereas where broad coalitions form, the effect (if any) is sustained, and that any effects are more likely where governments secure parliamentary majorities. These results show that the success of the model in Britain owes much to the fact that the economy was always an issue in the postwar period, and that elections routinely return single-party majority governments. In the comparative analysis, however, it was the countries where coalitions form and predominantly sustained effects appear that produced the most significant estimates, so Hibbs's original model is also supported in other contexts.

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[26] Both in Canada and in Germany the sustained model produced equilibrium shifts upward associated with shifts to the right in 1972, although these were insignificant. The sustained model for Norway produced much the same pattern of effects as the transitory, although with worse fit. Note also that, in Denmark and Sweden, the insignificant coefficients nevertheless follow the expected direction of effects.

These results have both economic and political significance in a number of ways. First, and most obvious, the results are clearly inconsistent with the strong policy ineffectiveness propositions of some modern macroeconomic theorists. Not only do political effects on unemployment exist, but it appears possible to sustain them. Whether the governments in which these sustained effects appeared lasted long enough to count as evidence against long-run policy ineffectiveness is uncertain. However, the key points are that political effects exist and that a priori political-institutional arguments explain when such effects appear and how they work.

Moreover, these results enrich simple neoclassical models of unemployment by systematically including other effects, most notably those of political interventions and external shocks. The results extend Hibbs's original party control hypothesis by including the effects of world demand and the possibility of transitory impacts. The effects of changes of party control are found in a context which more adequately represents the constraints the world economy imposes on policymakers in countries with open economies. This too still allows specific predictions of behavior to be made and tested.

Of course, claims should not be exaggerated. The tests included in this article do not exhaust the determinants of unemployment whose systematic effects could be sought. Nor do they exhaust possible measures of partisan impact on economic policy (Castles, 1982). Indeed, only a superficial model of the international transmission of demand shocks is provided. More theoretically informed equations for the impact of external shocks would be desirable. But this is a beginning. The systematic manipulation of fiscal and monetary policy, and the impact this manipulation has on unemployment net of the constraints imposed by external influences and of the early-term promisekeeping of new incumbents, await further study.

Within political economics, the results are important in other ways. They imply some degree of public control of economic policy, because promised policies appear to be carried out, at least initially. There is also evidence here that political effects on economic policy, even those rooted in reelection motives, come after rather than before elections. Political business cycle theorists have often failed to find significant effects on instruments or outcomes before elections (see, e.g., Alt & Chrystal, chap. 5; Beck, 1983; Paldam, 1979). Although specific tests for such effects were not included in the results reported above, investigation of residuals from the models reported in Table 5 indicates no systematic or remarkable departures from randomness before elections. The study of interparty differences and post-election behavior will benefit theorists interested in political effects on the business cycle.

Furthermore, the findings echo a major theme of modern political economics, that parties are organizations of political entrepreneurs who make strategic calculations even while implementing policies that are in the interest of their supporters (see, for instance, Alt & Chrystal, chap. 2). The evidence on "mandates" indicates that partisan effects do not arise automatically from class interests. Partisan effects are confined to times when the economy was publicly discussed during election campaigns or coalition negotiations. Perhaps the economy only becomes an issue when class interests are particularly at stake. If so, that class interests did not appear to be at stake in half the transitions is an important modification to the party control hypothesis.

Moreover, evidence of transitory impacts in two-party systems is also consistent with viewing parties as groups of strategic agents who use public policy to build electoral coalitions. Of course it is impossible to exclude the possibility that transitory impacts resulted from the effects of economic constraints not included in the model. However, although all the countries studied were part of the Bretton Woods system, not all shared Britain's chronic balance-of-payments problems. More important, the elasticity of response of domestic unemployment to the state of world demand was a direct function of openness, rather than (for example) the most open countries (by implication, most constrained) being the ones to display transitory effects. In the short run at least, the domestic authorities in these countries were unable (or did not try) to offset the effects of external influences.

This conclusion must be qualified and clarified in two ways. First, the fact that domestic authorities could not or did not systematically offset the short-run effects of world demand does not mean they could not do so in crises or at particular times. The government of a country with large public employment can use the public sector to damp or delay the impact of a decline in demand for the country's exports, although to do this it must either incur productivity losses in manufacturing or pay a subsidy to some sectors financed by general taxation, or both. Nevertheless, relative to the domestic level of unemployment, no factors could be found that explain which, if any, governments have done this systematically.

Second, there are still the long-run differences between countries noted above. Cameron (1978) shows that countries with open economies have relatively high levels of industrial concentration, worker organization, socialist participation in government, and public participation in the economy. The Phillips Curve pattern of the 1950s and 1960s, along which countries were ranked by socialist participation in government (Hibbs, 1977), broke up in the 1970s (Alt & Chrystal, 1983, chap. 5). But why do three of the countries with the most open economies, Austria, Finland, and Norway, still have among the lowest rates of unemployment, whereas Belgium, the Netherlands, and Ireland have the highest levels?

No single factor, like duration of right or left governments or frequency of government intervention in the economy, explains these differences. A speculative answer is that sustaining low unemployment against world trends requires suppressing sectoral conflict among workers, so that public action does not produce selective benefits to competing sectors. A high degree of worker organization is necessary to demand reduced unemployment; also required is a (socialist) government disposed to supply it. What is also necessary is a labor movement sufficiently all-embracing to act as a monopolist, securing and enforcing all bargains across the board, and to prevent the loss of aggregate benefits through conflict among worker organizations. A labor movement that can act in such a way is one whose membership is broad, including both manual and nonmanual workers; in which factions are absent; and in which one organization dominates the field. Data in Schmitter (1981) show that the labor organizations of Austria, Norway, Denmark, Finland, and Sweden are the most characterized by associational monopoly, whereas those of Belgium, Ireland, and the Netherlands are far less so.[27] Countries that began the period with low unemployment, with labor federations that could function as monopolists, have preserved comparatively low unemployment levels. Those with fragmented labor federations have incurred larger long-run increases in domestic unemployment, even though this factor does not explain differences in short-run responses.

The models of this article have short-term impacts with longer-term consequences. The dynamic model estimates show that the effects of changes in world demand linger for several years. Even transitory policy inputs have effects that are present and discernible after two or more years. Most important, these differences in economic outcomes justify continuing to seek political models of the manipulation of economic instruments by which these changes in unemployment were achieved. In this article I show that there is a politics of economic policy, both as agenda-building and institutional differences, even after one controls for a great deal else.

Appendix

Unemployment rates can be expected to display autoregressive behavior over time. However, some determinants of unemployment have shifted "permanently," and these permanent changes have caused the equilibrium level of unemployment to increase. When such changes accumulate, the unemployment series is said to be "integrated," and such series must be "differenced" to achieve stationarity before attempts are made to estimate autoregressive parameters. In addition, monthly data series contain a lot of local noise, some of which results from regular annual patterns of, for example, labor supply and demand. Seasonal adjustment removes much of this noise.

Minimally, the purpose of the analysis in this article is to isolate and estimate two regular components of the shocks (or two transitory sources of disequilibrium), world demand and changes of party control. These will be sought independent of the processes ordinarily changing unemployment, which will be modelled using ARIMA techniques. The purpose of the ARIMA modelling is to reduce an observed series (called "realizations" or "manifestations") to a white noise or random series ("innovations") plus a set of processes by which the random input becomes the observed series. The models are conventionally described by the notation (p,d,q) where p is the order of the autoregressive, q the moving-average process (if any), and d the degree of differencing required to achieve a stationary series. Seasonally dependent monthly series are often adjusted by applying a $(P,D,Q)_{12}$ model multiplicatively with the main model. A model that contains an $AR(1)$ parameter, is differenced once, and seasonally adjusted by applying an $MA(12)$ parameter to twelfth differences is denoted $(1,1,0)\times(0,1,1)_{12}$. The noise term in equation (1) for British data is of the form $(1,1,1)\times(0,0,1)_{12}$. In the form of equation (1), the equation used for estimating is:

$$U_t = \frac{b_w}{(1-d_wL)(1-d_0L)}W_t$$

$$+\sum_i \frac{b_i}{(1-d_iL)(1-d_0L)}G_{it-1}$$

$$+\frac{\Theta+\mu L+\epsilon L^{12}}{(1-\phi L)(1-d_0L)}a_t$$

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[27] The odd result is Denmark, which should have sustained relatively lower unemployment than it has. The period of tax revolt involved governments unwilling to pay the subsidy costs. The monopolistic view of Danish labor is criticized in Schwerin (1980).

with d_0 set initially to 1.0, μ and ϵ are moving-average parameters, and ϕ the autoregressive parameter. The estimation of a quasi-differencing parameter is a compromise between unnecessary risks of eliminating true partisan effects by misspecified differencing and inclusion of irrelevant terms through not differencing.

Parameter values are estimated by nonlinear least squares. The algorithm used takes advantage of the fact that the autoregressive process $y_t = bx_t + dy_{t-1}$ may also be represented by the infinite sum $y_t = b \sum_i d^i x_{t-1}$. A finite approximation of this sum may easily be programmed as a loop, in which first-order dynamic processes involving discrete changes (intervention analyses), continuous inputs (transfer function analysis), or past errors (autoregression) may be accumulated. Moving-average terms are estimated by adjusting the errors before accumulation.

For instance,

```
ell = e;te = phi*le;to=dO*lo;ema=mu*ell;
tg = bl*G + dl*lg;
tw = bw*W + dw*lw;
lg=tg;lw=tw;
uh = theta + te + tg + tw - ema;
e = u - uh;a = e - ema;le = te + a;
uh = theta + te + tg + tw + to;
o = u - uh;lo = to + o; retain le e lo lg lw;
```

is a loop sufficient to estimate a first-order transfer function on *W*, intervention for *G*, and *ARMA*(1,d_0,1) process on the random errors "a." This loop (plus initial conditions and seasonal adjustments) can be entered into the *SAS-NLIN* procedure to give nonlinear estimates and asymptotic confidence intervals of the model described above.

The procedure by which estimates were actually obtained is as follows.

Step 1. Initial parameter estimates for the noise term were taken from preliminary SAS-ARIMA estimates, with d_0 set to 1.0, while all other impact parameters were started at .01 and all dynamic parameters started at .5. Each model was estimated using a nonlinear minimization procedure.

Step 2. The resulting residuals from Step 1 were examined for evidence of non-randomness. With any necessary adjustment made to the noise term (and small alterations made to other coefficients to avoid local minimum problems), the model was rerun.

Step 3. If the residuals from step 2 were not white, step 2 was repeated once more. Then each model was re-run, if necessary, with any insignificant noise term parameters omitted.

Step 4. As confirmation, the final solution was re-run with a different minimization routine. If major discrepancies were observed, analysis returned to step 2.

Finally, some comments on the results. Every care has been taken to generate sensible estimates without data mining, including, for example, the use of multiple procedures. Some problems remain. In particular, the estimated political effects could be exaggerated, for two reasons. First, their estimated standard errors are approximate (Ralston & Jennrich, 1978), and could be underestimated. Second, the specification of world demand is not ideal. Some form of structural estimation might improve estimation of world demand effects, and thereby give a clearer picture of the size of partisan effects. Not much can be done about this at present.

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Bureaucratic Expertise versus Legislative Authority: A Model of Deception and Monitoring in Budgeting

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The empirical literature on the control of bureaus notes that politicians have difficulty observing bureaucratic output, but this insight is rarely represented in formal models. To analyze how bureaus use this uncertainty strategically, we develop a model of expertise-based agenda control, building on the Niskanen (1971) and Miller and Moe (1983) tradition. We show that under some plausible conditions, bureaus will underestimate *the benefits, and* overestimate *the costs, of their programs. In the model, politicians are neither passive nor omniscient: they anticipate the bureau's strategic behavior and establish a monitoring system to counteract it. This possibility of detection changes the bureau's behavior: even imperfect monitoring reduces the bureau's deception of the legislature, whether or not the legislature's demand for the bureau's services is concealed. Moreover, uncertainty by itself matters: if the legislature makes it harder for a risk-averse bureau chief to predict demand or penalty, the bureau will restrain its deception.*

> Under normal conditions, the power position of a fully developed bureaucracy is always overtowering. The "political master" finds himself in the position of the "dilettante" who stands opposite the "expert" . . . (Weber in Gerth and Mills, p. 232).

Since Weber, expertise has generally been considered the foundation of bureaucratic power. Although politicians have the formal authority to direct nonelected bureaucrats, their de facto power is weakened by two informational problems. First, observing the output of a bureau is often difficult. For example, while national security is the service that the Defense Department provides the country, in peacetime it is hard to estimate how secure the United States actually is (Stockfisch, 1973). Second, bureaucrats typically possess special insight into the relation between programs and consequences. When legislators ask such questions—"Does compensatory education really work?" "What kinds of welfare programs impair work incentives?" "How will the new weapon system perform?"—they often realize that the bureau is likely to know more than they or other outsiders (Fenno, 1966; Wildavsky, 1979).

Plainly, this expertise gives bureaus strategic opportunities. It is said, for example, that the Corps of Engineers has overstated the benefits of projects in order to increase the chance of receiving approval from Congress (Ferejohn, 1974), that the FBI has exaggerated the severity of crime waves in order to receive larger budgets (Cook, 1964), and that finance departments in city governments underestimate future revenues in order to resist political pressures for more spending (Larkey & Smith, 1983). Such examples of manipulation could easily be multiplied; they are all made possible by information asymmetries between bureaus and legislatures.[1]

But if agencies can get things they want because they know or claim to know facts other actors do not, do they actually use this asymmetry in sys-

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[1] Following the stylization common in this area, mainly for reasons of mathematical tractability, we ignore the three-way interaction among bureau, legislature, and president. The two-actor information asymmetry we deal with here could just as easily be taken to represent the interaction between bureau and president. It may also be noted that budgets are only one type of outcome shaped by the asymmetrical information and authority relations between bureau and political overseer. For a model in which the bureau has a mission orientation and uses its expertise to try to persuade its political superior to approve policy alternatives consistent with that mission, see Bendor, Taylor, and Van Gaalen (1984).

tematic ways? The fundamental theoretical issue here is the nature of agenda control; that is, how bureaucrats influence the choices of their superiors.

Two Types of Agenda Control

It is useful to distinguish two types of agenda control by their source of influence. (See Miller & Altfeld, 1984.) What we shall term "type 1" agenda control is influence over outcomes that derives from delegated authority over the choice set, such as when a legislative committee uses its formal authority to sit on alternatives it dislikes rather than let the whole legislature vote on them. "Type 2" agenda control derives from expertise, such as when one of the armed services forwards biased evaluations of proposed weapons systems to the Secretary of Defense.

Type 1 control has been the focus of much research on purely internal legislative behavior. However, an early and influential model of bureau-legislative relations of the type 1 variant was offered by William Niskanen in 1971. Niskanen modelled the budget decision-making process between legislature and bureau as a simple bilateral monopoly relation. Theorists later recognized that in Niskanen's model the bureau's ability to present legislative committees with a take-it-or-leave-it budget offer was equivalent to full agenda control. Subsequent research explored how different agenda rules shape legislative outcomes. In Romer and Rosenthal's (1978) model, for instance, the bureau's agenda control is much less complete: the policy reverts to the status quo if the bureau's alternative does not win acceptance. (See also Denzau & Mackay, 1982; Mackay & Weaver, 1981; and Shepsle & Weingast, 1981.)

Type 1 agenda control makes sense in a legislative setting; it is less persuasive as a paradigm for bureau-legislative interactions. A bureau's influence rests not on its formal authority—its ability to manipulate the rules—but, as Weber noted, on its control of information, its ability to manipulate either the construction of policy alternatives or information about the consequences of different alternatives. But although type 2 agenda control is a more empirically persuasive starting point, it is only recently that bureau manipulation of information has been given a central place in formal models (Miller & Moe, 1983; Spencer, 1982).

A Framework for Explicitly Considering Uncertainty

Interestingly, the issue of the political superior's uncertainty about the bureau's performance was recognized by Niskanen, but has not been highlighted in bureau budgetary models since he initially framed the problem. In Niskanen's pioneering *Bureaucracy and Representative Government* (1971), a bureau exchanges a lump-sum budget for a promised amount of output. The model assumes that this promise must be made good: the bureau must deliver the quantity of output it claimed it would, a constraint implying that the lump-sum budget must equal or exceed the total costs of the promised output. The reason the promise is binding is fundamentally informational: Congress is presumed to know precisely how much output is in fact produced, either because it can perfectly and costlessly observe the true amount of output or because it has a complete grasp of the bureau's production function. Niskanen, a former official of the Bureau of the Budget and a shrewd observer of government, acknowledged that this informational assumption was problematic:

> Bureaucrats and their sponsors do not, in fact, talk much about output—in terms of military capability, the value of educational services, the number and condition of the poor, etc. Most of the review process consists of a discussion of the relation between budgets and activity levels, such as the number of infantry divisions, the number of students served, the number of poor served by a program, etc. The relation between activity level and output is usually left obscure and is sometimes consciously obscured. . . . For those readers who have trouble with the concept of the output of a bureau, the theory developed in this book may be better understood in terms of the relations between budget or cost and the activity level, *rather than the output of a bureau.* The activities of a bureau, however, should be recognized as intermediate services which are valuable only as a function of their effectiveness. (pp. 26-27)

This qualification, however, comes early in the book, and all the formal models that follow interpret supply as final output. More important, the research tradition that followed Niskanen largely continued this practice of blurring the distinction between a bureau's activity level and its output. Consequently, this modern deductive tradition has deemphasized the Weberian problem of the informational foundations of bureaucratic power. To join the insights of the older Weberian tradition with the analytical power of the more recent formal deductive tradition, we propose a model of legislative-bureau relations in which Congress' difficulty in assessing how much it is getting for its money is accorded a central place.

Specifically, we assume, following Niskanen's distinction, that Congress knows exactly how much intermediate services it is getting for its money, but it does not know the production func-

Figure 1.

Budget → Intermediate Activities → Programmatic Output

tion—the relation between this activity level and output.[2] (See Figure 1.) (Since Congress knows the cost function for intermediate services, knowing the production function—i.e., how intermediate services are transformed into final output—is equivalent to knowing the cost function for final output and vice versa.) The bureau's superior expertise is embodied in its knowledge of this relation between intermediate and final output. The agency uses its informational advantages to try to secure larger budgets from the legislature.

As Miller and Moe (1983) point out, however, it is unlikely that politicians will passively submit to manipulation. Instead, they will use their superior authority to counteract the bureau's informational edge. Hence, we agree with Miller and Moe that to treat this relation as an ordinary bilateral monopoly is empirically unsound. First of all, Congress is the superior; the bureaus are subordinate. Whatever strengths the bureaucracy has lie not in its formal authority, but in its expertise. In terms of authority, Congress holds all the cards. Accordingly, in our model Congress uses its superior authority to organize the sequence of moves in the budgetary process. Specifically, the bureau must go first by announcing price-quantity and, hence, budget-output combinations. This is in contrast to Niskanen's model, and Miller and Moe's demand-revealing situation, where a bureau waits for Congress to reveal its demand before it announces its marginal cost function, a sequence that is clearly advantageous to bureaus by allowing them to use the demand information to construct public supply curves that suit their purposes.

Second, like Miller and Moe, the bureau must offer a per-unit price schedule, for example, *x* dollars per tank. Unlike Niskanen-type bureaus, it is forbidden to present a take-it-or-leave-it offer to Congress, for we agree with Miller and Moe that real bureaus would not dare to challenge their superiors by such hardball tactics of agenda control. Third, Congress unilaterally decides on a budget and appropriates it to the bureau. The bureau cannot bargain with the legislature.

All three properties—the sequence of moves, the kinds of alternatives the bureau is allowed to offer, and Congress' unilateral choice of a final budget—represent the institutional context of the relationship that makes it a special type of bilateral game. These properties reflect how Congress actively uses its institutional levers in order to control, ex ante, the bureau's ability to exploit its advantage of expertise. Furthermore, in our model Congress also has ex post instruments of control in the form of a monitoring and penalty system (to be described later). The combination of these ex ante and ex post controls transform the legislature from a passive object of bureaucratic manipulation into the shaper of the relationship.

These controls represent the advantages of authority deployed by the legislature to offset the superior expertise of the bureau. In the first part we highlight a pure version of this struggle between the resources of expertise and authority by positing a one-to-one relation between resources and institutions: all authority rests with Congress, all informational advantages with the bureau. Thus, the bureau's superior knowledge is uncontaminated by any uncertainty regarding the legislature's demand for its services or the actual cost of supply: it knows both perfectly. In the second section we relax these strong assumptions, thereby lessening the asymmetry between the bureau and the legislature, and examine the play of expertise-based agenda control in this more realistic setting. In the third section, we give Congress still more control by allowing it to allocate its monitoring resources across bureaus.

The goals of the two antagonists remain the same in all three parts. Congress seeks to maximize the net benefits of the bureau's program, the gross benefits minus the budget.[3] If Congress knew the true price of output, it would maximize net benefits by directing the agency to produce at the point at which marginal benefits equalled marginal costs, where the demand curve represents

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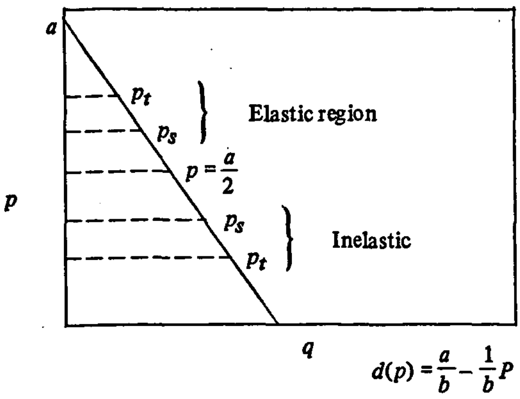
[2] More technically, we assume monotonicity: Congress knows that increases in activity level increase output, but it does not know by how much.

[3] We assume that Congress as a whole deals with the bureau, or, equivalently, that a perfectly representative committee is in charge. In such a world the maximization is in terms of the median legislator's cost and benefits. In note 13, we relax this assumption by introducing a high demand committee.

This median legislator interpretation is quite standard. However, reducing a complex institution to the preferences of a single member does require rather strong assumptions about the dimensionality of the issue and about the institutional structure. Fortunately, the basic results of the model do *not* depend upon the median legislator interpretation. Indeed, congressional demand and the monitoring system could be the resultant of complex, decentralized political forces, aggregated in the legislative arena in subtle ways. All that is required, explicitly in the second part and implicitly in the first part, is that the bureau have a probabilistic understanding of these resultants. (See note 9, for further discussion.)

Figure 2. p_s **and** p_t **in Elastic and Inelastic Regions**



An explanation is owed readers puzzled by the contrast between the demand curve in Figure 2—which can be read off the fiture as $p = -bq + a$—and the equation used in the text—$q = d(p) = \frac{a}{b} - \frac{1}{b}p$: the latter form is called the inverse demand curve and is traditionally used to show the dependence of quantity demanded on price, since price is the control instrument and thus the variable we differentiate by; however, by long custom among economists, followed in microeconomics textbooks, the graphical representation reverses the axes.

the marginal value of output. For its part, the agency wants larger budgets. It is constrained, however, by the threat of a penalty that the legislature will impose should its deception be uncovered.

In the next section, we describe the basic model in more detail.

The Basic Model

The Steps of the Budgetary Process

Relying on its superior authority, Congress asks the bureau to reveal the production function by revealing the true unit cost, p_t, of the programmatic output q. Congress intends to use this information to compute, from its own demand function, how much output is desirable, and hence the bureau's budget.

In this section, we assume that in responding to the request for a unit price, the bureau knows perfectly Congress's demand function for output. This demand for output, q, is a simple linear function of price, $q = \frac{a}{b} - \frac{1}{b}p$. Voters are assumed to want more of a public good the less it costs. Because reelection incentives induce similar preferences among politicians, the slope of the legislative demand curve is negative ($-\frac{1}{b} < 0$); for convenience, we also assume it is constant. The budget is thus the price, p, that Congress accepts, multiplied by the congressional demand for output, q, at that price, or $b(p) = p \cdot q$.

In replying to the Congress' request for a cost schedule, the bureau states that the unit cost is p_s. For convenience we will call the difference between the bureau's stated unit cost, p_s and the true unit cost, p_t, the bureau's "bias" β, i.e., $\beta = p_s - p_t$. (See Figure 2.)

(It should be noted that owing to these relations among price, output, and budgets, the bureau's claims about costs imply equivalent claims about output: if a bureau understates costs, it is thus simultaneously *over*stating the benefits that can be attained with any fixed budget. Thus readers more engrossed by what bureaucracy produces than by what it costs can translate all of our conclusions about the latter into conclusions about the former.)

We assume that Congress has the foresight to anticipate that bureaus are tempted to misrepresent costs and benefits to obtain larger budgets. To deter this behavior it establishes a monitoring system, so that with probability s, $0 \leqslant s \leqslant 1$, it discovers the bureau's deception and the true cost of output. Naturally, this monitoring is costly. (To assume that monitoring is free is not only unrealistic, it would trivialize the problem, for then Congress, by deploying unlimited monitoring, could always ensure that s equalled one. This wipes out the information asymmetry which is the heart of the bureau's agenda control. It is easy to show (see below) that in such circumstances the bureau, knowing it will be caught, quotes the true price.)[4]

If the bureau's deception is discovered, Congress imposes a penalty proportional to the absolute amount of the bias: the penalty function $\pi(p_s)$ is $c|\beta|$, where β is the amount of price distortion and c the magnitude of the penalty per dollar of bias. Because penalizing the bureau budgetarily might hurt Congress by reducing output, we conceive of the legislature as choosing to punish the bureau in some nonbudgetary way. We visualize the penalty taking the form of closer supervision, harassment, public embarrassment and, ultimately, damage to the offending bureau-

[4] Here we do not consider how the legislature would optimize on its allocation of monitoring resources. Clearly, the calculation would depend on the expected marginal benefits and costs of monitoring. In the third section we allow the legislature to allocate monitoring effort, given a total resource constraint, across bureaus.

crats' careers.[5] However, although the legislature may reduce the costs of imposing the penalty (by making it nonbudgetary), and shift some costs to interest groups, we assume that it always costs *some* decision makers time and effort to implement the penalty. As with monitoring, to assume a costlessly implemented penalty is counterfactual and trivializes the problem.

In short, the agency must trade expected budget against expected penalty. The bureau can find itself in either of two states with quite different payoffs. The first situation is that Congress does not discover that the stated price p_s differs from the true price p_t. This blissful situation occurs with probability r. The budget is then based on the bureau's stated price, $b(p_s)$ and there is no penalty. If the bureau were certain that this would happen, of course, it would simply set p_s to maximize its budget.

However, the bureau realizes that Congress may, with probability $s = 1 - r$, discover that it has been deceived. In this second situation, Congress assesses a penalty against the bureau and substitutes its own optimal budget, the budget based on the true price, $b(p_t)$, for the budget based on the bureau's stated price, $b(p_s)$. The budget based on the true price maximizes Congress' net benefits.

The bureau anticipates the legislature's reaction to biased statements by taking into consideration the two aforementioned possibilities—a budget $b(p_s)$ and no penalty with probability r and a budget $b(p_t)$ with penalty $c|p_s - p_t|$ with probability s—and chooses p_s to maximize the expected value of the linear utility function $u(B, \Pi) = B - \Pi$:

$$E(U) = E(B - \Pi) = r\,b(p_s) + s\,[b(p_t) - c|p_s - p_t|] \qquad (1)$$

Such an optimal stated price is the value of p_s where on average the marginal benefits of further increases in p_s just equal the marginal costs. Technically, the bureau's optimal p_s is found by solving the calculus problem involved in differentiating the above equation by p_s and setting it equal to zero:

$$E(\frac{\partial U}{\partial p_s}) = 0.$$

The solution, derived in the Appendix, is thus

$$p_s = \begin{cases} \dfrac{a}{2} - \dfrac{scb}{2r}, & \text{if } p_t \leqslant \dfrac{a}{2} \\ & \text{(the inelastic case)} \\ \dfrac{a}{2} + \dfrac{scb}{2r}, & \text{if } p_t > \dfrac{a}{2} \\ & \text{(the elastic case)} \end{cases} \qquad (2)$$

(It is understood—see Appendix discussion of equation (A3)—that even if the probability of deception being discovered is very high, the bureau would never be so foolish as to quote a price that would give it a budget lower than that it could obtain by stating the true price.)

Elasticity-determined Differences

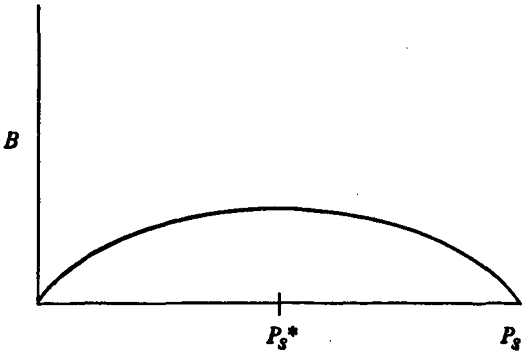
Equation (2) exhibits the first result: bureaus do not all behave the same way. Their differing responses depend on the elasticity of demand for their services.

Price elasticity measures how sensitive the demand for a good is to changes in its price. For small changes in price and quality, the elasticity of demand at a point on the demand curve is percentage change in demand divided by percentage change in price,

$$\frac{\%\text{ change in demand}}{\%\text{ change in price}}, \text{ or } \frac{\Delta q}{q} \Big/ \frac{\Delta p}{p}.$$

Demand is elastic if a price hike reduces the quantity demanded by a larger percentage than price itself rises; demand is inelastic if the proportionate decrease in quantity is less than the percentage increase in price; elasticity is unity if the two percentages are equal. Thus, when demand is elastic, revenue (budget) rises when price falls, since the percentage decrease in price is more than compensated by the percentage increase in quan-

Figure 3. Budget as Function of Price



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[5] These are interpretations. All that is really needed to drive the model is a penalty that has three properties: it is a nonbudgetary control, Congress can vary it continuously, and the bureau dislikes it. It may, of course, be hard to impose penalties; if this is true, then the model tells us what the effects of this incapacity are.

tity. However, if demand is inelastic, then revenue falls when price falls, since the percentage decrease in price exceeds the percentage rise in quantity. Note in particular that if demand is linear, as in Figure 3, then the top half of the demand curve is elastic, the bottom half inelastic.

The implications for a budget-hungry bureau are significant. If such a bureau's true costs are in the inelastic region of the demand function, it will overestimate its costs. Only if the bureau faces elastic demand will it quote to the legislature a price lower than the true unit price. This is a surprising prediction because most people think that bureaus systematically *under*estimate the price of their services. Perhaps they do, but this does not inevitably follow from the logic of utility maximization. In our models, bureaus facing inelastic demand overestimate the true price in order to enhance their revenues.

If the reader finds this implausible, recall that overestimating the per-unit price of output is equivalent to *under*estimating the amount of output citizens would receive for a given budget. Now reflect: is there any policy sector where critics of the agency charge that the bureau systematically underestimates true output for fixed budgets? Defense, of course. The level of national security is a function of the Department of Defense's activity level *and* the threat posed by the Soviets. Overestimating the Soviet threat implies underestimating the national security for any given budget. Stated in these terms, price overestimation appears quite plausible empirically. If the reader had initially believed that positive biases in defense were unreasonable, it is probably because he or she was thinking of individual weapons systems, which are indeed usually presented to Congress with suspiciously low price tags. But the demand for a particular weapon system is probably elastic, since partial substitutes exist. And when demand is elastic, the bias is negative. Since national security is considered a necessity, and since it lacks good substitutes, its demand is probably inelastic within a broad range.[6]

These effects of demand elasticity make intuitive sense, for elasticity reflects a seller's power over a buyer. Generally, if a good or service has many substitutes, buyers will be relatively sensitive to price (elastic demand). In the limiting case of perfect competition, there are so many complete substitutes that individual sellers have no power at all. If a seller raises his price by *any* amount, he loses all of his buyers to competitors; that is, demand is infinitely sensitive—elastic—to price. If, however, there are few substitutes, and the service in question is a necessity, then buyers may be relatively insensitive to price, giving a seller the power to raise revenues by raising price.[7]

We should note here an important asymmetry between bureaus that overestimate cost and those that underestimate it. The former are promising to do something feasible, whereas the latter are promising to do something that is *in*feasible—they know they cannot produce for less than true minimum cost. And since (if the bureau's deception is undiscovered) actual output equals $\frac{b(p_s)}{p_t}$, the promised output, q_s, is also infeasible:

$$q_s = \frac{b(p_s)}{p_s} > \frac{b(p_s)}{p_t} \text{ since } p_s < p_t.$$

Hence, they are declaring that for any given budget they will deliver an infeasibly large amount of output.

This behavior would never occur if Congress could costlessly observe output, or perfectly estimate it via knowledge of production functions, as in Niskanen's model. In such a world, the legislature would always observe that less output was produced than promised and would penalize the bureau accordingly. Hence, a bureau would never exaggerate the output it could produce. Owing to the equivalence of output overestimation and cost underestimation, bureaus would never understate costs; the stated price must imply a budget that covers the total costs of promised output. Therefore, if bureaus in Niskanen's model were stripped of their ability to present take-it-or-leave-it offers to Congress, then all agencies facing elastic demand would reveal the true minimum cost, p_t, since this yields the largest budget that still covers total cost (as can be seen by comparing pq budget rectangles in Figure 3). This approach illustrates the sensitivity of models of bureaus facing elastic demand to informational assumptions, a point noted by Breton and Wintrobe (1975).

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[6] The inclination of some bureaus to underestimate, whereas others overestimate, costs can be appreciated from a somewhat different angle. When demand is linear, the budget is a quadratic function of price: $b(p) = pq = \frac{a}{b}p - \frac{1}{b}p^2$. Differentiating by p yields a maximum budget, b^*, when $p = a/2$, the middle of the demand curve. Thus, revenues first rise, as p_S increases toward $a/2$, and then fall, once p_S moves past $a/2$. This indicates that if the true price is less than $a/2$, the bureau faces costs in the inelastic region of the demand curve, and if the true price exceeds $a/2$, demand is elastic.

[7] For evidence on differences in the elasticity of demand for public sector goods, see Borcherding and Deacon (1972) and Bergstrom and Goodman (1973).

Effectiveness of Monitoring: The Two Polar Cases

The foregoing elasticity-related results show how bureaus in different environments will have different optimal biases. But the environment of a bureau in either elasticity region can change, making the old optimal p_s no longer optimal. The method of comparative statics addresses this issue by computing how the optimum bias changes with changes in other key parameters. Technically, this simply involves differentiating equation (2) for the optimal p_s by the parameter of interest. (See the sets of comparative statics results in equations (A4) and (A5).)

In both elasticity regions, the bias is reduced with increases in the penalty severity c (i.e., $\frac{\partial p_s}{\partial c} > 0$ in the elastic case, $\frac{\partial p_s}{\partial c} < 0$ in the inelastic), and with increases in the probability of being caught s (i.e., $\frac{\partial p_s}{\partial s} > 0$ in the elastic region, $\frac{\partial p_s}{\partial s} < 0$ in the inelastic). Thus, in this preliminary case without demand uncertainty—even with full knowledge of Congress's demand—the bureau backs off from the budget maximization point, and the more so the greater the expected penalty.

The two polar cases of monitoring effectiveness are as expected. If the probability of its deception being detected is zero (i.e., $r = 1$, $s = 0$), the bureau will set p_s at the budget maximizing point. Thus in the case of nonexistent monitoring, hence zero expected penalty, the bureau is completely unrestrained. This is analogous to the Niskanen model, and to Miller and Moe's demand-revealing situation.

What happens in the opposite situation, where monitoring is very effective? As the comparative statics show, as the probability of its bias being detected, s, becomes greater, the bureau's bias becomes smaller. Interestingly, as s approaches one, the bias will become zero *before* s is equal to one.[8] Although we have no reason to suppose this is a general result, in this model the legislature's monitoring does not have to be perfect to eliminate the bureau's incentives to lie.

Finally, we note that the *types* of monitoring required in the elastic and inelastic cases are different. Formally, the monitoring technology presumes only that there is a probability s that the bureau's deception will be detected; it is silent on how this occurs or what exactly is monitored. All that the model *formally* specifies is that the Congress does learn the true unit price. If, however, one were to specify the *process* of monitoring, such as what variables were observed, then it would become apparent that there is an important difference in the situation of bureaus with elastic compared with inelastic demand. As Figure 3 shows, a bureau in the inelastic region will quote a price higher than the true price. Hence its total budget $b(p_s)$ will be larger than that actually needed to produce the output promised, the quantity q_s. It could actually produce the quantity of output q_t or more. In this case, legislative monitoring of output alone will not reveal the bureau's mischief; only monitoring the production process internally will uncover the slack.

The bureau in the elastic region is in a very different situation. Its utility-maximizing price is less than its true unit cost. Thus whenever it states a price less than p_t, it is promising to produce at an infeasible price. Although the budget given it, $b(p_s)$, is larger than the budget it would get if Congress knew the true unit cost, $b(p_t)$, the bureau cannot deliver the amount of output promised at the larger budget. Monitoring of the bureau's output will, with probability s, discover this shortfall. Monitoring of the bureau's internal process will presumably also reveal this shortfall.

These monitoring differences are a consequence of the fact that output is a sufficient indicator for the elastic but not the inelastic bureau. However, it must be realized that our model is silent on the question of how much output—within the feasibility constraint of the budget—the bureau chooses to produce with its appropriated budget. Because the model has no production function, it says nothing about the quantity of final output chosen by the bureau.

A Caveat: The Missing Production Stage

This point merits further discussion. It is important to clarify just what questions these models do and do not address. Our formal model is a theory of exchange, not production. The bureau promises to deliver a certain quantity of output in exchange for a budget; how revenues are transformed into programs is not explicitly represented. Hence we cannot say how excess budget is used, whether it is allocated to nonproductive slack (the "plush carpeting" syndrome), to produce more output ("After we maximize our budget, all we care about is production"), or to

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[8] This is seen by solving for the value of s at which $p_t = p_s$, the point where the bureau stops lying, which is

$$s = \frac{|p_t - \frac{a}{2}|}{|p_t - \frac{a}{2}| + bc}.$$

Inspection shows this is less than 1 whenever bc is nonzero.

invest in activities that do not increase *current* output but which may increase future output (policy analysis). This caution has normative implications. Although the model indicates the presence of socially excessive budgets, without a model of production one cannot conclude that *output* is excessive. Although it is tempting to make this inference, it is not supported by formal reasoning. Unfortunately, we believe that this inference is now part of the informal conventional wisdom that has developed around the edges of the formal modelling tradition. Most models in the Niskanen tradition represent exchange, not production, yet one often reads that the formal analysis of bureaus has shown that governmental output is supraoptimal.

Uncertainty and Cautious Bureaucrats

Demand Uncertainty

The above comparative statics results show that the ex post controls of a monitoring and penalty system can dampen an agency's tendency to exploit its informational advantages. There is another potential instrument available to the legislature: its own informational advantages. Just as it is reasonable to assume that bureaucrats know more about supply than do politicians so it is reasonable to assume that the latter know more about demand. Therefore, although in the first part we assume that the bureau knows demand perfectly, it is more realistic to assume that the bureau has only a probabilistic knowledge of legislative preferences.[9] In addition, if Congress does not reveal its demand function to the bureau, the agency is faced with the prospect of guessing wrong about Congress' response to its stated price p_S. The oft-made assertion that bureaucrats are cautious implies that this uncertainty is disquieting to the bureau. One might conjecture, as Miller and Moe do (p. 321), that the more Congress keeps the bureau in the dark about its own preferences, the more the bureau will toe the line and tell the truth.

We examine this conjecture by allowing the legislature to mask its preferences for the bureau's output, to conceal its demand in the hope that bureaucratic caution in the face of uncertain demand will restrain its tendencies to exploit its expertise. Here demand uncertainty is operationalized as $Q = q \cdot X$, where q is the deterministic component of demand, as defined in the basic model, and X is a random variable with a mean of one. Since the budget cannot be negative, X is greater than or equal to zero. (Graphically, uncertain demand can be visualized in Figure 3 as points scattered to the left and right of the demand curve, with the line itself representing the expected demand at any given price.) With this representation, demand becomes more uncertain when, loosely speaking, the distribution of X is more spread out, making extreme values of congressional demand (the highs and lows) more likely than before. Because the increased probability of high is exactly offset by the increased probability of low demand, the mean demand at any given price is unchanged.

(Increasing uncertainty, of course, covers the case of changing from certainty to some amount of uncertainty.)

With the bureau continuing to maximize expected utility, as in the basic model, the legislature gains *nothing* by concealing its preferences: the bureau does not change p_S in response to greater uncertainty about demand. Why not?

The reason is that there is nothing in this specification of the bureau that corresponds to caution. Recall the bureau's utility function: $u(B,\Pi) = B - \Pi$. Now consider a simple example of increased demand uncertainty: at first the bureau knows with certainty how much the legislature will demand for any given price; subsequently the legislature will want either 110% as much output, with probability of one half, or 90% as much output, with probability one half. Since $B = pq$, the bureau will get either 10% more budget as in the certainty case, or 10% less. Since the utility function indicates that utility is linear in the budget, that is, $u(kB) = ku(B)$, the uncertain budget yields exactly the same utility as the certain one: $E(u) = \frac{1}{2}u(1.1B) + \frac{1}{2}u(.9B) = \frac{1}{2}(1.1u(B) + .9u(B)) = u(B)$. A utility function of this type reflects *indifference* to risk, or risk-neutrality. If a bureau chief is cautious, he would prefer certainty to uncertainty, that is, he would be risk-averse.[10]

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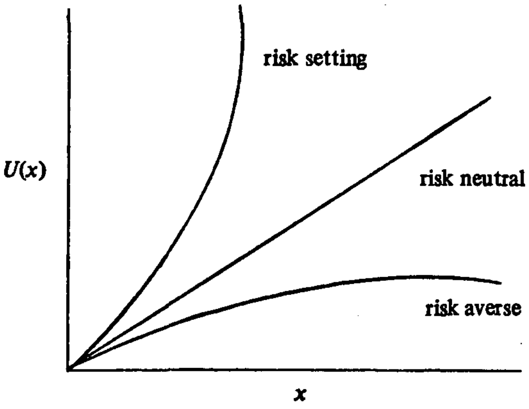
[9] Indeed, one might argue that the results on policy instability in pure majority rule institutions make even probabilistic knowledge problematic. How could a bureau predict legislative demand if majority rule can cycle over the entire policy space? However, matters are not that hopeless; legislatures are not that chaotic. Committee control of the legislative agenda can produce a structure-induced equilibrium (Shepsle, 1979). And even setting aside structural constraints, Ferejohn, Fiorina, and Packel (1980) have shown, using reasonable assumptions about the probability of one alternative beating another, that a pure majority rule institution reaches a limiting *distribution* of outcomes. Our assumption that the bureau knows Congress' demand function probabilistically fits well with the Ferejohn *et al.* result.

[10] Risk aversion is both an ordinary language term and a technical term in expected utility theory, where it measures attitudes toward risk. (For a lucid introduc-

Figure 4. Illustrative Utility Functions



Utility functions representing risk aversion are *concave* in the good in question (see Figure 4), reflecting the declining marginal utility of the good.

Only a slight modification of the original utility function is needed in order to incorporate the empirically reasonable postulate of risk aversion: now $u(B,\Pi) = B^{\phi} - \Pi^{\theta}$, with $0 < \phi < 1$ and $\theta > 1$, whereas the original risk-neutral function was $\phi = 1$ and $\theta = 1$. The new utility function depicts a bureau that is risk averse in both budget and penalty.[11]

When a bureau represented by this risk-averse utility function is subjected to increased uncertainty, it responds as expected: it reduces its bias (see discussion of appendix equation (A6)).[12] Thus, here we have an analogue to the Miller and Moe model. Increasing demand uncertainty is the continuous parameter equivalent of Miller and Moe's contrast between demand-revealing and demand-concealing behavior by Congress. The qualitative effect is similar: increased demand uncertainty helps Congress in this bilateral game. We emphasize, however, that this effect is produced by a combination of increasing uncertainty and bureaucratic risk aversion. Increasing uncertainty by itself is insufficient: Congress cannot make a risk-neutral bureau behave more docilely by obscuring its own preferences for output, because, intuitively, the bureau that faces an uncertain demand will simply take its best guess about the demand, and in the absence of any special assumption of error in this estimate, it will then treat the threat of losses equal to the possibility of gains, and then not react in any special way to the greater uncertainty. This is simply what is meant by risk neutrality. We belabor this point because the bureau's attitude toward risk does matter. If it is risk neutral, increasing uncertainty in demand will have no effect on its bias. If it were quite risk seeking toward the budget, for some reason, then in this model the bureau would increase its bias as demand uncertainty increased, for it would appreciate the possibility of getting away with bigger budgets more than the possibility of ending up with low budgets. In conclusion, insofar as we believe risk aversion, of a mild or massive degree, is the empirically more common attitude, then demand uncertainty aids Congress in its oversight.[13]

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tion to these concepts, see Hey, 1979.) Suppose an individual has two alternatives, both yielding the same expected value, determined by multiplying the possible payoffs by their probability. If one alternative produces a certain outcome, and the other yields either a better or a worse result, compared to the certain outcome, then a risk averse individual prefers the first alternative to the second, a risk-neutral individual is indifferent, and the risk-seeker prefers the second alternative. More formally, a decision-maker is risk-averse in a good, x, if and only if $u(E(x)) > E(u(x))$.

[11] When we describe a bureau as risk neutral or risk averse, a term usually describing individual attitudes, we mean this to be taken as shorthand for describing the attitudes of a bureau chief, whose risk preferences we assume dominate the hierarchical organization's choices. For an argument that organizations may have predispositions that are as stable as individuals', see Stinchcombe (1968, p. 184, note 16); for some evidence, see McFadden (1976, p. 70). As McFadden noted, one can use the method of revealed preferences to study organizations as well as individuals. Attitudes toward risk are merely one type of preference.

[12] The choice of utility function matters, as can be seen by using the appendix test on the utility function $u(B, \Pi) = -1/B - \Pi$. With increased risk, the bias $|p_S - p_f|$ decreases in the utility function used in the text, but increases with this new utility function. (See also Appendix on demand uncertainty.) For a model that uses more general functional forms, see Bendor, Taylor, and van Gaalen (1985).

[13] One matter of interpretation may be worth mentioning at this point. In our model the bureau deals with Congress as a whole, or with a committee that represents the floor perfectly. But bureaus normally deal with committees that want either more or less output than does the median legislator. How do our results change if we introduce unrepresentative committees into the model? A detailed answer would require another article, but the main effect can be explained briefly. Let us assume that the relevant committee controls the legislature's agenda in its jurisdiction, that is, only the committee can propose changes in the status quo (Shepsle, 1979; Shepsle & Weingast, 1981). If the committee does not report out a bill, the status quo is the default policy. Following the conventional stylized representation, let there be high-demand, middle-demand, and low-

Because the legislative strategy of increasing uncertainty is impotent if the bureau is risk neutral, we shall assume, in the rest of this part that the agency is risk averse.

Increasing Uncertainty in the Penalty

Increasing demand uncertainty is not the only way the legislature can make an agency labor under the disadvantages of informational asymmetry. Just as Congress may obscure its preferences for bureaucratic services, so too it may obscure its penalties for bureaucratic distortions. Indeed, it is plausible that Congress could manipulate this source of uncertainty more easily than it can increase demand uncertainty. It turns out that both manipulations have the same effect: a risk-averse bureau reduces its bias when the magnitude of the penalty becomes less known. (Since this can be shown in a manner completely analogous to the demand uncertainty case, we present the calculations in the Appendix.) Indeed, the effects of demand and penalty riskiness are additive. Increasing one type does not hinder the use of the other, and if one type is hard to increase, as demand uncertainty may be, increasing the other type can substitute as a disciplining force on the bureau.[14]

Thus far we have emphasized the increased uncertainty of demand and penalty as the result of deliberate actions of the legislature, steps taken to ensure that the politicians' ignorance about programs' costs and benefits is countered by bureaucrats' ignorance about their superiors' preferences. It is possible, however, that the demand for their services could become more opaque to bureaucrats as a by-product of legislative acts directed at other matters. For example, by lessening the power of committee chairmen, the changes in the House rules in the early seventies probably made final floor votes harder to predict. Of course, the main thrust of the changes in rules was to redistribute power in the House itself, rather than to gain greater control over the bureaucracy. Nevertheless, the comparative statics just discussed indicate that cautious bureaus would engage in less expertise-based agenda control as their own environment became less certain in the seventies. Thus, exogenous changes can put the two institutions on a more even footing by subjecting the bureaucracy to greater uncertainty.

Exogenous forces such as more volatile inflation or more rapid technological change could also make costs and benefits less predictable. Accordingly, the extreme informational asymmetry of the model in the first part is now transformed into a somewhat more symmetric—and more realistic—relationship: the bureau continues to know more about supply than the legislature does, for it has an unbiased estimate of p_t, but it cannot predict program costs perfectly. It is time, therefore, for us to relax the strong assumption of the basic model that the bureau knows its own supply function with certainty.

Supply Uncertainty

We now assume that the true price is a random variable, P_t, and investigate how a risk-averse bureaucrat responds to *increases* in supply uncertainty, a change that includes as a special case moving from the certain costs of the basic model to any degree of uncertainty. Intuition suggests that a cautious decision maker would decrease his deception.

We continue to use a linear penalty function, and, since the results are symmetrical, we assume that we are in the inelastic region, and that the bureau has the same risk-averse utility function used in the demand uncertainty case. For mathematical tractability, we also use the deterministic demand function with which we started.[15]

As always, the bureau's best-stated price is that price that makes the expected marginal benefits of increased deception just equal the expected marginal costs. Since the penalty is a function of the bias, and the bias equals $p_S - P_t$, the increased randomness in P_t makes the penalty more uncertain, which in turn affects the marginal costs of

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demand groups of equal size in Congress. Now suppose that after an election, the high-demand group gains control of the committee, replacing the low-demand group. The committee now prefers greater output at every price level. If the status quo policy is one of low budget and output, the committee can propose a higher budget-output combination than the median legislator will prefer (Shepsle & Weingast, 1981). Thus, the change in the committee's composition can be analyzed as a shift in the *effective* demand for the bureau's services. But the effect of a change in demand on the bureau's bias is just one of the comparative static results of equation (2). Assuming for convenience that shifts in demand are changes in the slope of the demand curve, keeping the intercept constant, equation (2) shows that the bureau increases its bias in response to increases in demand.

[14] Although we have not proven it, our hunch is that, if the domain of the demand and supply random variables were suitably bounded so that P_t never changed from one elasticity region to another, all three sources of increased uncertainty—demand, supply, and penalty riskiness—would be additive and substitutable.

[15] Furthermore, and crucially, we assume that P_t stays in the inelastic region. If it can cross elasticity regions, all results are indeterminate. For similar reasons, we cannot ascertain results when demand and supply are both uncertain.

deception. But the marginal *benefits* of p_S are unaffected by P_t's randomness since these benefits derive only from $b(p_S)$, the budget Congress appropriates before it gets a signal about P_t. Therefore, we need only examine how the marginal costs of p_S change as P_t becomes uncertain. If these costs increase, the marginal utility of P_S will become negative at the old level of deception, and the bias will decrease.

The resulting equation, where u_2, u_{22} and u_{222} are, respectively, the first, second, and third partial derivatives of utility with respect to penalty,[16] is

$$\text{changes in marginal utility} = s\,u_2\pi''' + 2s\,u_{22}\pi'\pi'' + s\,u_{222}\pi'^3 \tag{3}$$

Rewriting the right-hand side in terms of the utility function $u(B,\Pi) = B^\phi - \Pi^\theta$, in order to see the conditions on θ more clearly:

$$s[-\theta\pi^{\theta-1}]\pi''' + 3s[-\theta(\theta-1)\pi^{\theta-2}]\pi'\pi'' + s[-\theta(\theta-1)(\theta-2)\pi^{\theta-3}]\pi'^3. \tag{4}$$

With a linear penalty function (i.e., $\pi'' = 0 = \pi'''$), and $\theta > 2$, we get the expected result: the marginal utility of p_S is negative, and thus increased riskiness in P_t reduces the bias.

But there is also a surprise concealed here. Intuition is only partly right. Risk aversion in the penalty holds whenever θ is greater than 1, yet when $1 < \theta < 2$—still risk aversion—we have the counterintuitive result that the bureau *increases* its bias as the uncertainty of the true price increases. Only if $\theta > 2$ does the bureau behave the way most people expect from risk aversion, which means that mere risk aversion will not produce the expected result in this model; the risk aversion must be sufficiently great to work.[17]

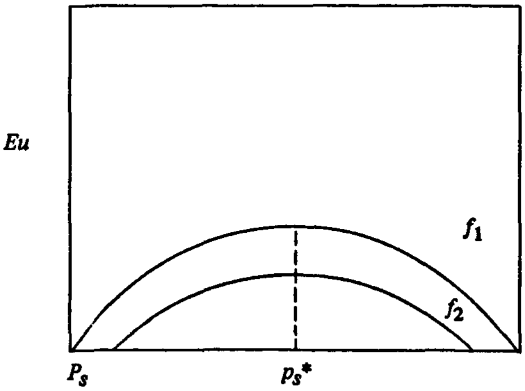
We are not quite done with surprises. If the bureau were risk seeking in the penalty ($0 < \theta < 1$), it would *decrease* its bias in response to increasing uncertainty in P_t. What is the explanation? Why would a risk seeker behave more cautiously after his environment becomes riskier, while a certain class of risk averters becomes more aggressive?

We think these results surprise people for two reasons. First, one may confuse the effects of changes in utility with the effects of changes in marginal utility. Second, one may misunderstand the nature of the bureau's problem. These two confusions mislead intuition about how risk-averse and risk-seeking decision makers will react to increasing uncertainty. We believe that eliminating these underlying confusions will change intuitive expectations so that they will not conflict with the model's formal predictions.

The two misunderstandings are related, but let us first consider the roles of marginal utility and of utility. Recall that the bureau maximizes expected utility by stating a price that equalizes the expected marginal benefits of deception with its expected marginal costs. Therefore, the fundamental quantity is marginal utility, not total utility. Suppose, for example, that the environment of a bureau became less benign. The effect on the bureau's welfare is depicted in Figure 5. Note that for every value of its decision variable, the bureau is worse off than before. But the slope of its utility function (marginal utility) is unchanged. Accordingly, whatever amount of deception balanced expected *marginal* benefits and costs in the old environment will do so in the new. The bureau will not change its behavior.

More generally, utility and marginal utility need not change in the same direction in response to a parameter's shift. That is precisely what is happening to bureaus in the $1 < \theta < 2$ and $0 < \theta < 1$ regions. For example, a risk-seeking bureau may become better off as P_t becomes riskier.[18] But that

Figure 5.



where $f_2 = f_1$ − constant,

so that $\dfrac{\partial f_2}{\partial p_s} = \dfrac{\partial f_1}{\partial p_s}$

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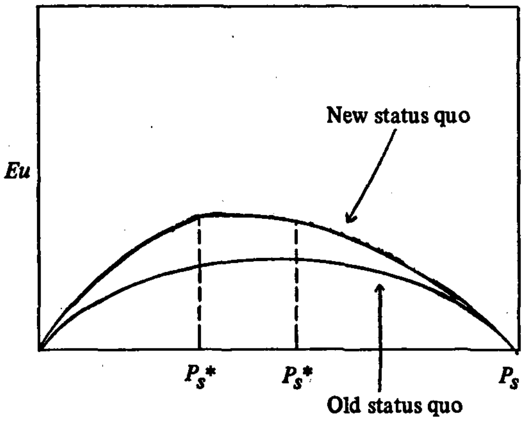
[16] The reader unfamiliar with our notation for derivatives of the utility function, for example, u_{222}, should consult the first section of the Appendix.

[17] Alternatively, as inspection of equation (4) shows, if the penalty is sufficiently more than linear, that is, p'' sufficiently great, then increased riskiness in P_t will reduce the bias for all degrees of risk aversion.

[18] More precisely, when demand is linear, the bureau must be *sufficiently* risk seeking for this to obtain.

Figure 6.



only means that its *total* utility increased; the *marginal* utility of deception could have decreased—and so it did. (See Figure 6.) The bureau is better off at all values of its decision variable in the more risky environment, but the new curve would be steeper than the old: deception's (expected) marginal costs have risen. Since marginal costs have increased while marginal benefits have stayed constant, the bureau reduces the bias.

The reader may believe that this example of utility increasing and marginal utility decreasing is bizarre, theoretically possible but empirically irrelevant. Should not we normally expect the two to move in the same direction? We think this belief is incorrect, and that it derives from an overly narrow interpretation of the kinds of choice situations confronting decision makers. Utility and marginal utility may change consistently in one class of choice situation but inconsistently in another. Let us call a situation where the decision maker can *escape* from uncertainty an "allocation problem"; a situation where escape is infeasible we call a "trap." In the prototypical allocation problem, a decision maker is allocating a fixed resource (money, time) to two alternatives, *A* and *B*. Again, the decision maker is risk averse. Alternative *A* is riskier than *B*, but the decision maker apportions some resources to *A* because it has a higher mean. Now suppose *A* becomes riskier still, while its mean stays the same. Naive intuition tells us that the decision maker will shift resources from *A* to *B*. Provided we make a few additional (rather mild) assumptions about the utility function (Rothschild & Stiglitz, 1971, p. 72), intuition is correct, because in *this* kind of circumstance utility and marginal utility do change in the same direction: both fall as *A* becomes less certain. Hence intuition can reach the correct conclusions even if it focuses on the wrong variable, that is, on utility.[19]

But the bureau is not facing an allocation problem. It is trapped: it cannot escape from uncertainty. If its environment becomes more uncertain, it must live with it, and we believe there is no reason to expect marginal utility and utility to change consistently in traps. The following example helps clarify why the risk-seeking bureau decreases its bias. Suppose a hardened convict can obtain more perquisites (cigarettes, drugs) by extorting them from weaker felons. If the extortions are detected, however, his sentence will be extended. The convict is risk seeking in the penalty. He is currently serving a 20-year sentence with certainty. Now a parameter changes: a new parole policy. The new policy is riskier, and the convict now has an even chance of serving one more day (i.e., essentially 0 years) or 40 years. We suggest that it is intuitively plausible that the con would extort less under the new regime. The marginal costs of extortion (doing additional time) could well increase if one has a 50-50 chance of getting out in only one day. Note that although the marginal utility of extortion has fallen, the con's *total* utility has increased at all settings of his decision variable—as a gambler, he prefers the riskier policy.

Analogously, although the risk-seeking bureau (of the $0 < \theta < 1$ situation) prefers the riskier environment, its marginal costs of deception have risen. (See Figure 6.) Accordingly, the bureau reduces its bias.

Changes in Risk Aversion

Thus far we have shown that the bureau's risk aversion interacts with changes in its environment (demand, supply, penalty function) to produce different degrees of exploitation of its informational advantage. We should also note that risk aversion by itself matters: if an uncertain environment's degree of uncertainty stays constant but the bureau becomes more averse to risk, it decreases its bias.[20] While perhaps rare, socially and institutionally modulated changes in attitudes toward risk are not unknown (Douglas & Wildavsky, 1982; Thompson, 1980). Changes in personnel policies, for example, can alter the population of individuals from which bureau chiefs are selected, and this new pool may exhibit different attitudes toward risk. For example, one argument for instituting the Senior Executive Service classification in the Civil Service was that it would attract more innovative (i.e., risk-seeking) decision makers to key positions in the bureaucracy.

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[19] An allocation problem becomes a trap when all alternatives worsen, for then there is no exit. In the example in the text, if alternatives A and B are the only feasible options, and they both become riskier, the decision maker's utility declines, but since the *marginal* returns of the investment in A and B remain equal, the decision maker does not change his behavior.

[20] Since the mathematics is tedious and provides no substantive insight, we omit the proof, which may be found in greater generality in Bendor, Taylor, and Van Gaalen (1985).

Summary

Uncertainty matters when a bureau is not indifferent to risk. Because bureaucrats are rarely described as risk neutral, it is important that a model of legislative-bureau budgetary relations be able to parametrize uncertainty. The factors we have analyzed—demand, penalty, and supply—all can change in riskiness in plausible ways, either as the result of deliberate strategic changes on the part of the legislature or as a by-product of other changes. Thus it is significant that—within the very simple model we have presented—increased uncertainty in demand, penalty, and, with a sufficiently cautious bureau, uncertainty about its own costs, all operate to reduce the bureau's temptation to ask for excess budgets. The overarching point is a comparative one: when the stark asymmetry of information between bureau and legislature in the basic model is made more realistic by imposing some uncertainty on the bureau, a risk-averse bureau is likely to deceive the legislature less.

Congressional Allocation of Monitoring Resources

Thus far we have assumed a fixed probability of detection and penalty. Although comparative statics on equation (2) can tell us how the bureau would respond to changes in monitoring, we have not yet explicitly analyzed how Congress would allocate its monitoring resources in the first place. We do that now.

We assume that Congress has resources, in the form of staff, the General Accounting Office, and similar agencies, that it can allocate across different policy sectors in order to monitor bureaus with varying intensity.[21] Thus Congress is in effect choosing s_i, the probability that it will detect the cost-benefit distortions of bureau i, and since the expected penalty is just $s_i \pi = s_i c(p_s - p_t)$, the legislature is in effect choosing the magnitude of the expected penalty that bureaus will face: if two bureaus distort the true price by the same amount, the one that is more closely monitored confronts both a higher expected penalty and a higher expected marginal penalty.

How the legislature allocates its monitoring resources depends on the marginal costs of observation in different sectors and on the marginal benefits of finding out what a given bureau is doing with its budget. For simplicity we hold marginal costs of observation constant across policy sectors and focus on the latter. How, then, might a bureau use its budget? We conceive of a bureau as being able to allocate its revenues to three uses: 1) producing more output; 2) embodying resources in an inefficient production process in order to please a powerful constituency; and 3) buying perquisites—what we earlier called the plush-carpeting syndrome.

The benefits of monitoring depend on how a bureau is allocating its budget to these three uses: the more the bureau is deviating from the allocation that Congress prefers, the more important is monitoring. How might legislators rank these three uses? Although it is almost surely a domain-dependent judgment—with differences among, for example, peacetime military spending, welfare, and energy policy—we believe that members of Congress would generally agree on the following increasingly distasteful rank ordering: excess output, inefficient production to satisfy a bureau's constituency, and perquisites. Excess output is socially excessive, but it is also more of just what Congress wants, and nearly half of all legislators and voters want more than the median demand. Likewise, a bureau's adoption of a less than maximally efficient production process in order to gratify a constituency (e.g., Forest Service logging policies in areas with poor prospects of economical regeneration of timber (Taylor, 1984)), although less forgivable than increased output, yields benefits for legislators whose districts gain financially from inefficient production (Shepsle, Weingast, & Johnsen, 1981). Finally, perquisites are things that make life more pleasant for the bureau, but these have no value to congressmen. Hence, perks are the most offensive use of the budget.

Figure 7 depicts the three political loss functions. Political losses increase with increases in illicit expenditures. Lacking a strong reason to suppose that marginal losses are rising or failing, we assume linearity.

Now the joint equilibrium of legislative and bureaucratic choices can be understood. By monitoring bureaus with varying intensity, Congress affects the budget-seeking behavior of an agency

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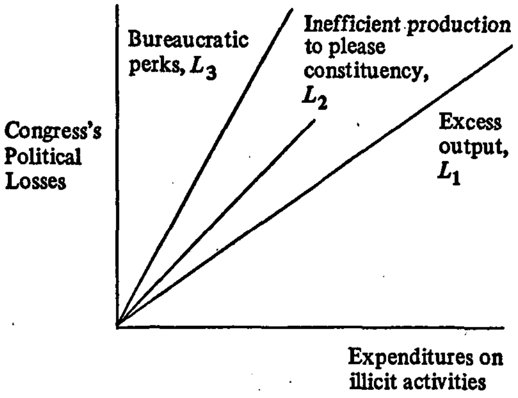
[21] Alternatively, one could model Congress' decision problem as the choice of its total monitoring budget, given a fixed governmental budget, rather than as the allocation of a fixed budget across policy sectors. Both models represent a similar problem of maximization under a resource constraint; they differ on the specification of the constraint.

Figure 7. Political Loss Functions



in the first period: the more oriented a bureau is to uses that elicit close scrutiny, the higher the marginal cost of another dollar of excess budget obtained by deception. Hence, ceteris paribus, such a bureau will state a smaller bias than will a bureau whose preferences for revenue allocation are more consistent with the legislature's. Thus the legislature's allocation of monitoring intensity that bureaus expect to encounter in the *second* period shapes the relative sizes of the budgets in the *first* period. In equilibrium, the first and second stages of decision making are linked in the interdependent choices of the two parties.

Ordinarily one would expect a bureau to allocate budget to all three uses to just that extent that the marginal benefits of the different end uses were equal. For analytical simplicity, however, let us consider only three "pure" types of bureaus, those caring about only one particular use of revenues, and whose derived utility functions are otherwise identical, say, as in the second part, $u(B,\Pi) = B^{\phi} - \Pi^{\theta}$. How might the legislature's different political valuations of potential misuse of funds affect its monitoring investments? We assume that Congress knows the bureau's type but, as usual, does not know the bureau's cost function.

For each bureau, the probability of detection s_i is determined by m_i, the amount invested in monitoring it: $s_i = g(m_i)$, where g is an increasing concave function ($g' > 0$, $g'' < 0$) on the assumption of a decreasing rate of improvement with increasing investment as one approaches the maximum probability of 1. Congress is assumed to allocate its monitoring budget M ($M = \Sigma m_i$) optimally among the bureaus, reflecting the influence of s_i on the bureau's stated price and output propensities.

The net benefits that Congress gets from a bureau can be conveniently summarized in net benefit functions f_1, f_2, and f_3 that reflect the output levels, the budget allocation, and political losses from each of the three bureaus' activities.[22] We assume that the f_i are independent of each other, and are concave increasing functions of the m_i, satisfying

$$f_1(m) \geqslant f_2(m) \geqslant f_3(m) \text{ and}$$

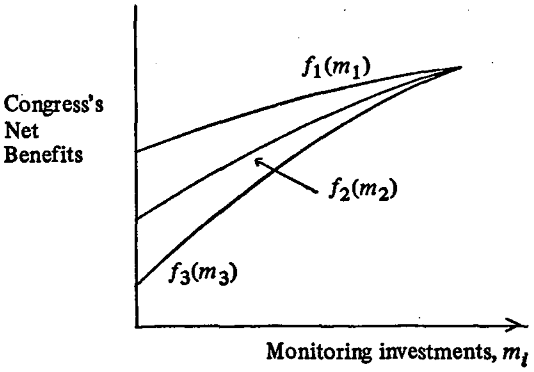
$$0 < f'_1(m) < f'_2(m) < f'_3(m)$$
$$\text{for every fixed } m$$

(The last two properties follow from the assumption that the bureaus produce different loss functions, as in Figure 7, but are otherwise identical. The concavity of the f_i follows from the concavity of the monitoring function g.)

Figure 8 shows the three net benefit functions. Then the optimal allocation satisfies $m_1 < m_2 < m_3$, since optimality requires $f'_1(m_1) = f'_2(m_2) = f'_3(m_3)$. Hence $s_1 < s_2 < s_3$. Graphically, the optimal allocation of M is represented by allocations of m_i which produce equal slopes of tangents to the three benefit functions.

This simple model shows how different political valuation of agency misuse of funds produces, in equilibrium, different degrees of monitoring of the bureaus. Such differential monitoring of different types of bureaus has implications for the equilibrium budgets, efficiencies, and outputs of agencies. The key normative question here is whether budgets and output are larger or smaller than what is socially optimal. In the context of

Figure 8. Net Benefit Functions



Note: The $f_i(m)$ converge because $p_s \rightarrow p_t$ as $s_i \rightarrow 1$.

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[22] We suppress budget as an argument of $f_i(b,m)$ on the assumption that Congress can compute its optimal demand independently of computing its optimal monitoring.

our analysis this breaks down into two issues: Is the bureau carrying out the legislature's policy? Is the legislature's choice itself socially optimal? We address only the first issue.[23] As a point of comparison, we note that the best remembered conclusion of Niskanen's model is that bureaus gain budgets and produce output in excess of what the median legislator would choose and what is socially optimal as well—and this result holds even when legislative committees impart no bias to the process. In our model do the budgets and output of agencies always exceed what a fully informed legislature would choose?

We contend that the answer depends critically upon 1) what an agency does during the implementation or production stage, that is, how it chooses to produce its service and how to allocate its budget among the competing uses of output, perquisites, and other possible goals, and 2) how Congress allocates its monitoring resources. Because 1) and 2) are interdependent in equilibrium, different types of agencies will exhibit different patterns of over- and undersupply. To simplify analysis, we continue to examine only pure types of bureaus.

Let us first compare a bureau that cares only about output with a bureau that cares only about perks. An output-oriented bureau will quote a larger bias than a perk agency because its expected marginal penalty for deception is smaller. Thus it will get a larger budget. Since it cares only about output, it will produce with maximum efficiency. Hence its output will exceed the amount that a fully informed legislature would choose.

The budget of the perk-loving agency will also be more than an informed legislature would want, although less than the output-loving agency. Like the output agency, the perk bureau will also produce at unit cost p_t, since inefficient production merely reduces the budget available for perquisites. But it will produce less output than the output-oriented agency for two reasons. First, it devotes a smaller percentage of its budget to output, since some of its budget goes for perks (Niskanen, 1975). Second, and less obviously, since the expected marginal penalty for dollars spent on perks is greater than for those spent on excess output, the bureau has stated a price closer to p_t, and hence its budget is smaller to begin with.[24]

Notice the contrast with the popular image that connects a large bureau with inefficiency: Because of the way monitoring works, an output-loving agency is efficient but it also lies more (because its deception is treated more leniently) and will therefore wind up with a bigger budget than a pure perk-loving bureau. Thus, it is among the class of efficient bureaus that one finds the bureaus that produce the largest output *and* consume the most revenues.

A constituency-sensitive bureau is willing to allocate its budget in the form of less than maximally efficient production, in order to please an important interest group. What will be the equilibrium tendency of its budget, production, and output? Because the expected penalty of the constituency-sensitive agency is higher than that of the pure output bureau, its requested budget is smaller (although it exceeds the perk agency's), and although all its budget is used for output, the output is produced at more than the lowest feasible unit cost, p_t, so the equilibrium level of output will be less than for the pure output agency: not only is the budget smaller, but whatever budget is devoted to output is converted to output less efficiently. But because the level of embodied inefficiency adopted by the constituent-sensitive bureau may be large or small, one cannot say whether its output will be above or below the amount the legislature would choose.

The foregoing analysis is one way an equilibrium may be established among legislature and bureaus. But empirical studies suggest other possibilities as well. In our model the legislature makes only postmonitoring cuts; but Congress can also threaten premonitoring cuts in an agency's budget request, and such a possibility may induce more caution in a budget-hungry bureau (see Bendor, Taylor, & van Gaalen, 1985). More generally, scholars of the budgetary process emphasize the crucial role of reputation. To quote Fenno:

> As agency officials recognize, prime among the virtues which count [with congressmen at appropriations time] are an essential honesty and frankness. One of the [Appropriation] Committee's influential leaders expressed the Committee's emphasis on the face-to-face virtues in this revealing analogy: "Would your wife buy

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[23] For the second issue, see, for example, Shepsle & Weingast (1984), and the references cited therein.

[24] Should the reader wonder whether a pure perk-loving agency would produce any output at all, recall that because the legislature knows the cost function of the bureau's intermediate activities, the bureau must deliver the quantity of intermediate activity implied by the appropriated budget. Thus, even if the perk-loving bureau degrades the *quality* of the intermediate activity in order to free up some revenues for perquisites (e.g., the highway is built with too much sand in the concrete), the fact that it must produce the intermediate good or service ensures that some amount of final output will be generated. In any case, such pure types of bureaus are empirically rare. Normally, one would expect bureaus to derive utility from generating a mix of final output and perquisites.

> groceries from a man she didn't like even if she could save a little money? No. You might ask her why she didn't trade at [the supermarket] instead of down at the corner and she'd say, 'I don't like those people. I got some bad groceries once.' Well, it's just the same with us. Some administrators come over here and lie to us. We cut them just to show them they can't do that." (Fenno, 1966, p. 340)

As the Fenno quotation suggests, legislative monitoring and bureau behavior combine to create agencies' reputations. In time, monitoring should reveal that some agencies are more trustworthy than others, in the sense of spending money in legislatively approved ways. These reputations influence and are influenced by bureau's budget requests, budgetary allocations, and legislative scrutiny. Agencies that enjoy good reputations are able to operate with greater autonomy, are safer from morale-damaging investigations, and are reasonably sure that they will receive a high (and more predictable) percentage of their budget requests. In equilibrium, the confidence of politicians is repaid: agencies with good reputations protect their good names by living up to them.[25] For its part, the dominant coalition in such agencies tries hard to avoid resource allocations (such as perks) that would soak up revenues without benefitting important legislative constituencies. Because these efforts are usually successful, monitoring rarely discovers gross malfeasance. Hence, the good reputations of bureaus are perpetuated; the beliefs of politicians, confirmed. The relationship is an equilibrium of expectations and actions.

Once we recognize that the formation of beliefs is an integral part of the process, it becomes clear that in a more adequate theory, reputations would be formed endogenously. This would require a multiperiod model—a promising direction for future research.

Conclusions

The model presented here suggests some quite different predictions from those of previous deductive models in this area.

First, bureaus do not always overstate their costs and understate their output. Rather, the direction of their cost/performance bias depends on the kind of demand situation in which they find themselves. Also, because it is likely that Congress will learn in which elasticity region the bureau operates, legislators will eventually learn the direction of bureaus' biases.

Second, bureaus' willingness to deceive legislators is constrained by the likelihood of detection and the penalty for deceit, both of which can be manipulated by the legislature. Furthermore, monitoring does not have to be perfect, or even nearly perfect, to have a disciplining effect on the bureau's propensity to seek excess budget.[26]

Third, increased uncertainty about the legislature's demand for the bureau's services, or about the penalty for deception, makes even mildly risk-averse bureaus more chary. Relatedly, different bureaucratic recruitment procedures can have a similar effect on the stated bias by changing the bureau's degree of risk aversion.

From our equilibrium analysis we see that although budgets are supraoptimal, output is not necessarily so. Moreover, the final consumption preferences of bureaucrats matter: they shape how much budget they seek and the output they produce. These systematic variations create different political opportunities for would-be reformers. For example, consider one implication of our equilibrium argument for a president wanting to cut the fat out of bureaus. A president who believed in the stereotypically bloated bureaucracy would expect to be able to find a lot of fat—budget that mattered only to bureaucrats—across the board. A president who believed our model could expect to find fat mainly in new (not yet equilibrated) programs or in policy areas that had recently experienced a large disequilibrating change in technology or demand—both of which may be relatively rare bureaucratic situations—and he would expect to find that the bureaus whose budgets were most excessive are precisely those bureaus with legislative and constituency backers who would not regard his proposals as merely cutting out fat. These presidents are in very different worlds.

Some political scientists have argued on informal grounds that theories attributing the growth of government to budget-maximizing bureaucrats are empirically incomplete. As Wildavsky observed,

> I wish to puncture the Parkinsonian proposition that bureaucrats expand their programs indefinitely by hoodwinking the population. Were these programs not deeply desired by strong social elements, they would not prosper. As

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[25] Note that this is a logical claim about the properties of an equilibrium. Whether such an equilibrium between politicians and bureaucrats obtains is, of course, an empirical question.

[26] Oversight works in our model even without threatening the monopoly status of the bureau. We suspect, however, that introducing bureaucratic competition would, as Niskanen (1971), Spencer (1982), Miller and Moe (1983), and Bendor (1985) have indicated, aid the legislature.

> Frank Levy told me, it is not the conspiracy theory but the Pogo Theory that is applicable. *We have seen the enemy and they are us.* (Wildavsky, 1980, p. 58, original emphasis)

This argument is an informal version of equilibrium analysis: Since politicians can make themselves better off by shifting some resources to monitoring bureaus that are absorbing revenues in ways that do not benefit powerful constituencies, it is unlikely that an enduring pattern of budgetary expansion is due to hoodwinking.

Although the Pogo Principle implies that one cannot explain the growth of the federal government from 4% of GNP in 1930 to 23% in 1980 on the basis of hoodwinking (i.e., bureaucrats exploiting informational asymmetries), hoodwinking may mean that the budget was higher than optimal at all times in that period. This could be true even if the growth had always been in equilibrium, because if it is costly to acquire information about programs, and if the information politicians receive from interest groups and policy analysts must be extracted from noisy signals, it is possible for informational asymmetries between politicians and bureaucrats to persist in equilibrium.[27] Consequently bureaucrats could obtain budgets in excess of what a fully informed legislature would choose, and in disequilibria, due perhaps to technological change, the asymmetries may be more marked.

These considerations of differential expertise have led us to regard the phenomenon of agenda control in a bureaucratic-legislative setting as being quite different from agenda-setting within legislatures. Rather than continuing to borrow from theories of agenda setting in legislatures, which naturally focus on authority as the source of influence, we have tried to be true to the basic empirical finding that bureaucratic influence derives from expertise.

Appendix

Notation

p_t, true cost per unit output
P_t, true cost when assumed random
p_s, stated unit price
$\beta = p_s - p_t$, bias
q, $d(p) = \frac{a}{b} - \frac{1}{b}p$, the quantity demanded as function of price
$Q = X \cdot q$, the quantity demanded when demand is random
B, the amount of budget (a random variable)
$b(p) = p \cdot d(p) = p \cdot q$, the budget function
r, probability that deceiving Congress goes undetected
$S = (1 - r)$ probability that deception detected
c, coefficient of penalty function
$\pi(\cdot) = c|p_s - p_t| = c\beta$ the penalty function
Π, the amount of penalty (a random variable)
q^*, optimal quantity of final output
p^*, optimal price of final output
$b(p^*)$, the optimal budget, that is, the budget at the optimal price
l_i, the political loss function, relating the amount the i^{th}-type bureau spends on illicit activities to Congress political losses
f_i, Congress' net benefit function from all the i^{th} bureau's activities
m_i, amount invested in monitoring the i^{th} bureau
s_i, probability of detecting the deception of the i^{th} bureau
g, the monitoring payoff function relating monitoring investment, m_i, to the effectiveness of monitoring, s_i.

For convenience we often use a simpler notation for derivatives of functions of more than one argument, in which $u_1(B,\Pi)$, or simply u_1, refers to the first derivative of the function with respect to the first argument of the function; for example,

$$u_1 \equiv \frac{\partial u}{\partial B},\ u_{11} \equiv \frac{\partial^2 u}{\partial B^2},\ \text{and}\ u_{12} \equiv \frac{\partial^2 u(B,\Pi)}{\partial B \partial \Pi}.$$

However, for single argument functions we sometimes use the "prime" form where the meaning is clear, so that, e.g., $b'(p)$ is $\frac{d(b(p))}{dp}$.

Assumptions of the Basic Model

1. linear demand function,

$$d(p) = q = \frac{a}{b} - \frac{1}{b}p,$$

hence quadratic budget function

$$b(p) = pq = \frac{a}{b}p - \frac{1}{b}p^2.$$

2. linear utility function, for the simple, risk-indifferent case: $u(B,\Pi) = B - \Pi$.

For risk aversion: $u(B,\Pi) = B^{\phi} - \Pi^{\theta}$, with $0 < \phi < 1, \theta > 1$.

3. linear penalty function, $\pi = c|p_s - p_t|$.

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[27] For the general point that noisy signals can support diverse beliefs in a rational expectations equilibrium, see Admati (1984, p. 2).

For further assumptions used in the variants in "Uncertainty and Cautious Bureaucrats," see appropriate sections of the appendix.

Optimal Bias and Comparative Statics

The maximand is expected utility,

$$E(U) = rb(p_s) + s[b(p_t) - c|p_s - p_t|]$$

$$= r\left(\frac{a}{b}p_s - \frac{1}{b}p_s^2\right) + s\left(\frac{a}{b}p_t - \frac{1}{b}p_t^2\right)$$

$$- sc|p_s - p_t|.$$

Differentiating by p_s and solving for the optimum, we get

$$0 = E\left(\frac{\partial U}{\partial p_s}\right) = r\left(\frac{a}{b} - \frac{2}{b}p_s\right) - sc[\text{sign}(p_s - p_t)].$$

The sign of $p_s - p_t$ is positive in the inelastic case, and negative in the elastic case. Thus, in the inelastic case,

$$\frac{\partial u}{\partial p_s} = 0 = r\left(\frac{a}{b} - \frac{2}{b}p_s\right) - sc,$$

and the optimal choice of the policy instrument p_s is

$$\frac{a}{2} - \frac{scb}{2r}. \qquad \text{(A1)}$$

And, similarly,

$$\frac{a}{2} + \frac{scb}{2r} \qquad \text{(A2)}$$

in the elastic case. Since it never pays the bureau in the inelastic (elastic) case to set p_s below (above) p_t, we have

$$p_s = \begin{cases} \max\left(\frac{a}{2} - \frac{scb}{2r}, p_t\right), & \text{if } p_t \leqslant \frac{a}{2} \ (\textit{inelastic case}) \\ \min\left(\frac{a}{2} + \frac{scb}{2r}, p_t\right), & \text{if } p_t > \frac{a}{2} \ (\textit{elastic case}) \end{cases} \qquad \text{(A3)}$$

To simplify the derivation of the comparative statics, we present a simplified version in the text, merely making explicit the proviso that the bureau never has an incentive to quote a p_s in the inelastic (elastic) region that is below (above) p_t.

The comparative statics, for example, $\frac{\partial p_s}{\partial c}$, $\frac{\partial p_s}{\partial s}$, are equally straightforward, especially if done on equations (A1) and (A2):

For the inelastic case:

$$\frac{\partial p_s}{\partial a} > 0, \frac{\partial p_s}{\partial s} < 0, \frac{\partial p_s}{\partial c} < 0, \frac{\partial p_s}{\partial b} < 0,$$

$$\frac{\partial p_s}{\partial r} > 0. \qquad \text{(A4)}$$

For the elastic case:

$$\frac{\partial p_s}{\partial a} > 0, \frac{\partial p_s}{\partial s} > 0, \frac{\partial p_s}{\partial c} > 0, \frac{\partial p_s}{\partial b} > 0,$$

$$\frac{\partial p_s}{\partial r} < 0. \qquad \text{(A5)}$$

Demand Uncertainty

Mathematically, we see if the bias increases or decreases with increased riskiness by determining whether $\frac{\partial u(B,\Pi)}{\partial p_s}$ is convex or concave as a function of the variable whose riskiness is increasing. The same method is used for demand, penalty, and supply uncertainty.

Demand uncertainty is represented by $q \cdot X$, where q is the deterministic component of demand, and X is a non-negative random variable, not a function of p, with $E(X) = 1$.

Substituting qX into $u(B,\Pi) = B^\phi - \Pi^\theta$, with $0 < \phi \leqslant 1$, $\theta \geqslant 1$, and finding the first-order conditions we obtain

$$E\left(\frac{\partial U}{\partial p_s}\right) = E\left[r\phi b(p_s)^{\phi-1} b'(p_s) X^\phi + s\theta\pi^{\theta-1}\frac{\partial \pi}{\partial p_s}\right]. \qquad \text{(A6)}$$

In the inelastic case, $b'(p_s) > 0$, so the expression in the square brackets is concave in X. Thus, by Jensen's inequality (see Lippman & McCall, 1981, p. 222, for an explanation in a microeconomics context), $E\left(\frac{\partial u}{\partial p_s}\right)$ decreases as X becomes riskier. It follows that p_s moves toward p_t to maintain $E\left(\frac{\partial u}{\partial p_s}\right) = 0$. Similarly, since $b'(p_s) < 0$, the bias decreases in the elastic case as well.

Assumptions: In our model, demand uncertainty is multiplicative. One cannot derive results here with additive uncertainty because one cannot determine whether $\phi b(p_s)^{\phi-1}$ in equation (A6) is concave in X. Overall, our results depend on four assumptions: Multiplicative demand uncertainty, where the random variable X is independent of price; a separable utility function ($u_{12} = 0$); a B that is deterministic except for Q in order to avoid overshooting the boundary of the elasticity region, which would make sign of $b'(p_s)$ indeterminate; and, to guarantee the existence of elastic and inelastic regions, linear demand.

Penalty Uncertainty

Analogously to demand uncertainty above, penalty uncertainty can be represented by $\pi \cdot Y$, where π is the deterministic component of penalty, and Y is a non-negative random variable, not a function of p, with $E(Y) = 1$.

Substituting πY and $b(p_s)X$ simultaneously into $u(B,\Pi) = B^{\phi} - \Pi^{\theta}$, and finding the first-order conditions we obtain

$$E(\frac{\partial U}{\partial p_s}) = E[r\phi b(p_s)^{\phi-1} b'(p_s) X^{\phi} + s\theta\pi^{\theta-1}\frac{\partial \pi}{\partial p_s} Y^{\theta}] \qquad \text{(A7)}$$

which is concave in both Y and X. Thus the effects of increasing uncertainty in both demand and penalty are additive.

Supply Uncertainty

Expecting out the randomness of P_t, total expected utility is

$$E(U \mid P_t) = r[u(b(p_s), 0)] + s[u(b(P_t), \pi(p_s - P_t))] \qquad \text{(A8)}$$

and the optimal p_s satisfies

$$0 = E(\frac{\partial U}{\partial p_s}) = E(\frac{\partial E(U \mid P_t)}{\partial p_s}) \qquad \text{(A9)}$$

Assuming that p_s is always larger than P_t in the inelastic case, that p_s is always smaller than P_t in the elastic case, and that P_t never crosses into the opposite elasticity region, what happens if the riskiness of P_t is increased with $E(P_t)$ unchanged? By Jensen's inequality, the effect of increased riskiness depends on the sign of

$$\frac{\partial^2}{\partial P_t^2} E(\frac{\partial E(U \mid P_t)}{\partial p_s}) = su_{222}\pi'^3 + 3su_{22}\pi'\pi'' + su_2\pi'''. \qquad \text{(A10)}$$

The signs of the components are:

For the inelastic case: $\pi' > 0, \pi'' = \pi''' = 0$ (with a linear penalty function). For the elastic case: $\pi' < 0, \pi'' = \pi''' = 0$.

Also: $u_2 = -\theta\pi^{\theta-1}; u_{22} = -\theta(\theta - 1)\pi^{\theta-2}; u_{222} = -\theta(\theta - 1)(\theta - 2)\pi^{\theta-3}$.

Thus: For $\theta = 1$: $u_2 \leqslant 0, u_{22} = 0, u_{222} = 0$. For $1 < \theta < 2$: $u_2 \leqslant 0, u_{22} \leqslant 0, u_{222} \geqslant 0$. For $\theta > 2$: $u_2 \leqslant 0, u_{22} \leqslant 0, u_{222} \leqslant 0$.

Now the sign of

$$\frac{\partial^2}{\partial P_t^2} E[\frac{\partial E(U \mid P_t)}{\partial p_s}]$$

is seen to be: for $\theta \geqslant 2$, negative for the inelastic case and positive for the elastic case; for $1 \leqslant \theta < 2$, just the reverse.

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Attitude Attribution:
A Group Basis for Political Reasoning

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This article shows that citizens can estimate what politically strategic groups—liberals and conservatives, Democrats and Republicans, and blacks and whites—stand for on major issues. These attitude attributions follow from a simple calculus, a likability heuristic. This heuristic is rooted in people's likes and dislikes of political groups. Thanks to this affective calculus, many in the mass public are able to estimate who stands for what politically, notwithstanding shortfalls in information and information processing.

The study of public opinion consists, with few exceptions, of the study of the beliefs and preferences of individual citizens. This article explores a new subject: not what individuals themselves believe, but what they believe others believe. Two questions concern us. First, to what extent can citizens estimate accurately the position of politically strategic groups on major issues? And, second, how can they figure out what such groups stand for, given how little they know about politics?

These two questions are worth considering. Citizens, it turns out, are remarkably accurate in estimating the issue positions of strategic groups in politics, including groups like liberals and conservatives about which one might well suppose the mass public to be ignorant. Given how much citizens do *not* know about politics, it is worthwhile establishing that there is an aspect of politics—and an important one—about which they *are* knowledgeable.

The analysis of attitude attribution, of how people estimate what others think, provides new insight into the workings of mass belief systems. The accepted view of their operation is, broadly, that most citizens do not understand political abstractions such as liberalism or conservatism and that they do not (because they cannot) put together a consistent, or constrained set of opinions on issues of the day (e.g., Converse, 1964, 1975; Levitin & Miller, 1979). But in what sense, then, do the beliefs of the mass public form a "system"? What is the glue that holds them together? What provides citizens with the means and motive to achieve a measure of coherence in their views about political issues, given how little attention they are likely to pay to politics and how little information about it they are likely to possess?

Our argument is that citizens can draw an impressively accurate map of politics, of who wants what politically, of who takes the same side as whom and of who lines up on the opposing side of key issues. Citizens can accomplish this, we argue, by relying on their political affect, their likes and dislikes of politically strategic groups.

We focus on the operation of an affective calculus, or, as we call it, a *likability heuristic.* This calculus is organized around people's feelings toward groups such as liberals and conservatives. Clearly, many in the mass public lack a firm understanding of political abstractions. All the same, many know whom they like, and, equally important, they also know whom they dislike. If coherent, these likes and dislikes can supply people with an affective calculus to figure out the

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Table 1. Attributed and Actual Positions of Liberals and Conservatives

	Liberals				Conservatives			
	Attributed		Actual		Attributed		Actual	
		N		*N*		*N*		*N*
Government guaranteed jobs (1976)	3.12	1127	3.91	286	4.92	1158	5.30	457
Rights of accused (1976)	2.87	1142	3.45	292	4.43	1160	4.61	479
Student unrest (1972)	2.73	761	3.62	195	5.23	767	5.40	250
Government inflation (1972)	3.51	653	2.41	184	3.62	689	2.59	275

issue positions of strategic groups. We suggest that in this way many in the mass public can figure out who wants what politically without necessarily knowing a lot about politics.

Public Perceptions of Liberals and Conservatives

Many, although not all, issues offer a choice between liberal and conservative positions, expressible in the familiar metric of the standard seven-point NES issue scale. In this metric, the smaller the number arithmetically (or the farther to the left graphically), the more liberal the position; the larger the number (or the farther to the right), the more conservative the position. After being asked to place themselves on these seven-point issue scales, respondents are asked to place others as well. In addition to being asked to locate the presidential candidates and the political parties on issues of the day, in 1972 and 1976 respondents were asked to locate a variety of groups, among them liberals and conservatives. Because respondents were also asked how liberal or conservative they themselves were, it is a simple matter (in principle at least) to compare the positions that liberals and conservatives characteristically take with the positions attributed to them.

Table 1 shows, first, the average position attributed to "most" liberals (and conservatives), then the mean position actually held by them,[1] on four issues: government guaranteed jobs, protecting rights of the accused, dealing with student unrest, and controlling inflation. Clearly, on all issues except controlling inflation, the public understands liberals to be on the left, although how far to the left varies somewhat, and they appreciate that conservatives are on the right. To this extent there is general understanding that liberals and conservatives represent *distinguishable policy alternatives* on a number of issues.

To be sure, citizens perceive liberals to be farther to the left on some issues than others, the most extreme of these being student unrest, and conservatives to be on the right on three of the four issues (again the inflation issue is an exception) and furthest to the right on the issue of student unrest. There is, then, a general understanding that liberals and conservatives represent distinguishable policy alternatives as well as an appreciation of which of them wants what.

The issue of inflation is one obvious exception, but the reason is obvious, too. The question asks respondents *if* government should try to control inflation, not *how* it should do so. As phrased, this question does not contrast liberal and conservative positions, and the results only reflect the fact that both liberals and conservatives dislike inflation even more than they are perceived to dislike it.[2]

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[1] Liberals are defined as respondents scoring 1, 2, or 3, conservatives 5, 6, or 7 on the NES ideology scale. To make the comparisons between attributed and actual positions we assume that the seven-point NES scales form inter-personally comparable interval scales. For every issue and group, about one-third of the respondents do not make placements, so that our discussion only applies to the remaining two-thirds.

Tables 1 through 3 are based upon the following sampling conventions: For 1972 the entire unweighted sample was considered (for a total of 2705), while in 1976 only those people who were interviewed in both the pre-election and post-election survey were considered (for a total of 1855).

Attributed Positions: Each attributed position is based upon all people in the sample who placed the group on the scale. Owing to the way the NES surveys were constructed, this automatically excludes people who do not place themselves on the issue scale. At the same time, it includes some people who do not identify themselves with one of the two groups being placed. For example, 15% of the sample place liberals and conservatives on issue scales even though they do not place themselves on the liberal-conservative identification scale. It is reasonable to include these people because it seems quite possible to know where liberals, conservatives, Democrats, Republicans, whites, or blacks stand without being in these categories.

Actual Positions: Actual positions include those people who place themselves on the issue scale and who identify with the group.

These rules lead to different numbers of people for almost every cell in these tables, but we have found no reason to suspect that this creates any systematic biases for the subsequent analyses.

Public Perceptions of Democrats and Republicans

These observations about the overall realism of ideological maps are not, of course, conclusive. Fortunately, the NES surveys also ask respondents to estimate the positions of the two major parties on an array of issues. Therefore, far from having to make a case based on four issues only, we have 10 issues to examine in 1976, and 15 in 1972. Moreover, all of the issues in the 1976 survey are in the 1972 survey, and observing consistencies in attitude attribution over time, as well as across issues, will buttress our initial impression that the effects observed are systematic.

The 1972 and 1976 NES surveys afford a generous representation of policy concerns: economic, welfare, racial, social, lifestyle, and even (in 1972) Vietnam. The results for the Democratic party are clearcut; plainly, the general public is aware of where Democrats characteristically stand on major issues. On nearly every one, they place Democrats on the left, where Democrats in fact are, judged either by policy statements of the party or by the attitudes of its partisans. Indeed, if anything, the general public perceives Democrats to be more liberal than they are.[3]

This awareness that the Democratic party is on the left does not reflect a blind judgment, made in the same way for every issue; for example, on the

Table 2. Attributed and Actual Positions of Democrats and Republicans in 1972 (NES 7-point scale)

	Democrats				Republicans			
	Attributed		Actual		Attributed		Actual	
		N		*N*		*N*		*N*
Government guaranteed jobs (1)	2.91	933	3.95	604	4.55	931	4.72	417
Tax rate (1)	3.42	810	3.85	594	4.49	825	4.22	418
Vietnam (1)	2.73	1003	3.28	632	4.46	1019	4.22	405
Marijuana	4.33	1589	5.28	1293	5.26	1639	5.47	863
Busing	3.83	1690	5.98	1275	4.91	1755	6.52	867
Government medical insurance	3.14	677	3.50	565	4.63	683	4.48	398
Pollution	2.73	814	2.16	599	3.09	844	2.16	399
Equality for women	3.08	1676	3.44	1302	3.51	1698	3.53	871
Vietnam (2)	2.70	817	3.21	533	4.54	847	4.09	392
Government guaranteed jobs (2)	3.19	748	3.98	489	4.42	762	4.84	331
Rights of accused	3.41	1366	3.92	981	4.17	1411	4.48	706
Minority aid	3.18	1499	3.96	1017	4.20	1521	4.55	722
Tax rate (2)	3.47	681	3.78	465	4.38	698	4.13	317
Urban unrest	3.07	738	2.91	481	3.95	741	3.38	331
Student unrest	3.77	727	4.53	470	4.69	747	5.26	332
Government guaranteed jobs (1)	2.90	1293	4.03	743	4.48	1288	5.00	549
Rights of accused	3.30	1147	4.06	760	3.95	1158	4.51	574
Busing	3.38	1181	5.90	827	4.10	1190	6.35	594
Minority aid	2.99	1228	4.12	757	4.13	1229	4.62	567
Government medical insurance	2.88	1046	3.52	707	4.71	1032	4.72	554
Government guaranteed jobs (2)	3.09	1254	4.24	711	4.72	1244	5.12	555
Urban unrest	3.05	1167	2.97	708	4.12	1154	3.65	512
Marijuana	4.26	960	4.74	793	4.70	972	5.03	562
Tax rate (2)	3.45	1155	3.90	772	4.56	1145	4.56	557
Equality for women	3.10	1135	3.16	823	3.58	1130	3.27	602

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[2] The responses to this question seem different from any other in a variety of ways. Accordingly, the inflation item is omitted from subsequent analysis.

[3] This left-shift appears to be an important asymmetry in the way that people perceive the left and the right, but proving its existence requires an extended examination of the properties of seven-point scales and a justification for our use of the "actual positions" in Tables 1-3 as the baselines for measuring where groups stand. We shall explore left-shift in another study.

Table 3. Attributed and Actual Positions for Blacks and Whites

	Blacks				Whites			
	Attributed		Actual		Attributed		Actual	
		N		*N*		*N*		*N*
Busing (1976)	3.59	1442	3.95	148	5.86	1506	6.32	1515
Rights of accused (1976)	2.39	1306	3.57	127	4.14	1366	4.35	1423
Minority aid (1976)	1.91	1405	2.44	144	4.71	1406	4.56	1396
Government medical insurance (1976)	1.73	1063	2.67	118	3.63	1272	4.16	1358
Government guaranteed jobs (1976)	2.36	1357	2.84	127	4.26	1378	4.78	1363
Rights of accused (1972)	2.63	1630	3.20	176	4.52	1661	4.27	1741
Government guaranteed jobs (1972)	2.36	839	2.05	88	4.43	850	4.56	857

issues of legalizing marijuana and busing, Democrats are not perceived as being as far to the right as they actually are. All the same, it is these two issues on which Democrats are—and are perceived to be—least liberal.

There is also an appreciation of the conservatism of Republicans and the Republican party. Thus, Republicans are perceived to be on the right on 20 of the 25 issues. Pollution and the role of women, two issues where they are perceived to be on the left, reinforce rather than contradict this impression of reality perception, because in these two cases, where there is agreement that Republicans are not on the right, they in fact are not, to judge from what Republicans themselves believe.

Nevertheless there is a qualification: although Republicans are understood to be on the right, they are not perceived to be so far to the right as Democrats are perceived to be to the left. Only on one issue, legalizing marijuana, are Republicans located more than one scale point from the right of the midpoint. In contrast, Democrats are located more than one point to the left on six issues. This difference should not be exaggerated: the mean position was 4.3, or approximately a third of a scale step from the midpoint; the mean position attributed to Democrats was 3.25, or three-quarters of a step. In a word, both are located on the correct side, although the Democrats tend to be perceived as further from, and the Republicans as closer to, the center.

Public Perceptions of Blacks and Whites

Not only do citizens, on average, accurately attribute attitudes for liberals and conservatives and Democrats and Republicans, but they also do so for a number of other groups, of which blacks and whites are an especially striking example. Table 3 shows the positions attributed to blacks and whites and those actually held by them on a number of issues in 1972 and 1976.

On all seven of the issues (counting 1972 and 1976) for which data are available, the general public perceives blacks to be on the left. In fact, blacks are on the left on all of these issues, with the exception only of busing, and, instructively, it is the very issue on which they are perceived to be least liberal.

Much the same is true for whites, except for the political direction being the other way around. Whites both are—and are perceived to be—on the right on most of these issues. This sense of white conservatism, though, is relatively muted, except for busing and minority aid, which refer explicitly to race. Again, the issue on which whites are least conservative—government medical insurance—is the issue on which they are perceived, correctly, to be most liberal.

In addition to this element of reality perception, attitude attributions for blacks and whites show signs of left shift: blacks are perceived to be more liberal, whites to be less conservative, than they are. This shift seems more pronounced for blacks than for whites, just as it was more pronounced for liberals than for conservatives and for Democrats than for Republicans. However this may be, the dominant finding is, again, the accuracy with which the general public can figure out the position of blacks and whites on issues, including nonracial issues.

Explaining Attitude Attribution

Determinants of Attitude Attribution[4]

To this point, we have focused on attitudes attributed to strategic groups by the general public taken as a whole. But of course there must be substantial differences in the attributions of attitudes *within* the public. After all, it would be quite remarkable if conservatives had exactly the same view of what liberals stand for as liberals themselves; remarkable because what people believe others believe must in some sense depend on what they themselves believe.

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[4] Tables 4, 5, and 6, it should be emphasized, are typical of those we obtained for many other issues, for both 1972 and 1976, and for Democrats and Republicans and blacks and whites.

Table 4 shows the positions attributed to liberals and conservatives, in 1976, by liberals and conservatives and, in addition, by moderates and by those unwilling (or unable) to describe themselves as either liberal or conservative. Clearly there is a connection between what people believe others believe and what they themselves believe. Just as clearly, the connection is far from straightforward. Thus, conservatives perceive liberals to be markedly more liberal than liberals perceive themselves to be, indeed, more liberal than they actually are. The reverse, however, is not true: liberals do not emphasize how conservative conservatives are; if anything, conservatives perceive themselves to be more conservative than liberals perceive them to be.

Why should conservatives emphasize how far to the left liberals are and also how far to the right they themselves are? Conversely, why should liberals minimize how far to the left they are and also not emphasize how far to the right conservatives are? Nor are these the only questions to consider. Notice the responses of "No Thought" respondents (Table 4, col. 4). They are willing to offer estimates of what liberals and conservatives stand for, but in what sense can they be said to have a meaningful idea of what liberals and conservatives stand for if they do not have a meaningful idea about whether they are themselves either liberal or conservative? They are, most probably, merely guessing, as the tendency of their estimates of both liberal and conservative attitudes to hover around the scale midpoint suggests. (The midpoint, after all, *is* the safest guess.)[5]

Moreover, it is surely not plausible to suppose that the "No Thought" respondents are the only ones who are guessing, which suggests in turn the usefulness of sorting respondents, according to levels of political knowledge, on the assumption that the more knowledgeable should be less likely to guess than the less knowledgeable. Accordingly, Table 5 presents perceptions of liberals and conservatives, by liberals and conservatives, using years of schooling as a surrogate for political awareness and cognitive complexity.

As Table 5 shows, conservatives spread the alternatives, that is, emphasize how far apart liberals and conservatives are on issues, and they are more likely to emphasize the difference between the two, the better educated they are. In a word, conservatives tend to accentuate *both* how conservative conservatives are, and how liberal liberals are. In contrast, liberals (unless they are well educated) tend to minimize how liberal liberals are. Nor is this to be viewed as politically strategic behavior on the part of liberals, an effort by them to capture the center; for liberals (again unless they are well educated) do not emphasize how conservative conservatives are.

This is peculiar: why should conservatives stress, while liberals (unless well educated) minimize, how far apart liberals and conservatives are? The most likely explanation is how they feel toward each other: dislike is a good reason to emphasize differences, and as Table 6 shows, there is a profound difference between liberals and conservatives in this respect.

Conservatives dislike liberals; how much is very much a function of how conservative they are. Thus, the least conservative place liberals at just under 50 degrees on a feeling thermometer,

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[5] One of the major points of this article is that many people "construct" their responses to questions based upon definite cognitive or affective cues including, sometimes, the "true" positions of groups. Guessers also use such cues, but they do not use any information about a group's true position. Equation (2) and Appendix 2 provide a detailed definition of what we mean by guessing.

Table 4. Attributed Positions of Liberals and Conservatives by Ideology (1976)

	Liberals		Moderates		Conservatives		"No Thought"	
		N		*N*		*N*		*N*
Of liberals								
Government guaranteed jobs	3.27	254	3.34	316	2.56	464	3.88	132
Rights of accused	2.95	263	3.04	323	2.44	406	3.54	134
Of conservatives								
Government guaranteed jobs	5.05	262	4.67	326	5.28	326	4.28	134
Rights of accused	4.70	266	4.19	329	4.64	411	3.93	134

Table 5. Attributed Positions of Liberals and Conservatives by Ideology and Education

			Liberals						Conservatives					
	Actual Position	*N*	0-11 Grades	*N*	12-13 Grades	*N*	> 14 Grades	*N*	0-11 Grades	*N*	12-13 Grades	*N*	> 14 Grades	*N*
Of Liberals														
Government guaranteed jobs	3.91	286	3.50	46	3.55	130	2.65	78	3.19	67	2.66	218	2.03	119
Rights of accused	3.45	292	3.10	48	3.11	136	2.59	79	2.72	60	2.57	219	2.10	126
Of Conservatives														
Government guaranteed jobs	5.30	457	4.81	48	4.87	134	5.51	80	5.18	67	5.17	222	5.52	123
Rights of accused	4.61	457	4.21	48	4.55	140	5.27	78	4.59	64	4.40	220	5.08	126

Measures of Ideology: Liberal (1,2,3), Conservative (5,6,7)

whereas the most conservative place them at a frosty 34 degrees. Similarly, liberals do not like conservatives; however, they do not dislike them nearly as much as conservatives dislike liberals. To be sure, the more liberal they themselves are—or the more educated they are—the less favorably they feel toward conservatives. Even so, given the same degree of ideology or schooling, conservatives dislike liberals far more than liberals dislike conservatives.

These findings are suggestive. One is tempted to conjecture that conservatives emphasize how liberal liberals are because they dislike them so. It is surely a natural response to see one's opponents in politics as more extremist the greater one's dislike for them. Liberals, however, do not repay the compliment: they tend to minimize how conservative conservatives are because they dislike conservatives less than conservatives dislike liberals.

In sum, how liberal (or conservative) a person perceives liberals and conservatives to be depends on whether he is himself liberal or conservative. In addition, it depends partly on how he feels toward liberals and conservatives. Finally, it depends on how much knowledge of liberals and conservatives he has, or conversely, how much he must resort to guessing. But how do these factors fit together? And, supposing more than one causal process is at work, which is the most important, which the least—or more exactly, which process is the most important for what kind of individual?

We argue that two major processes and two minor ones are at work. One of the major processes is reality perception. The other, a likability heuristic; that is, a calculus based on a person's beliefs weighted by his or her likes and dislikes. We think this heuristic is intriguing, because it suggests how, even if they do not necessarily know a lot about politics, people may nonetheless figure out who believes what. To see just how this heuristic is formed and how it operates alongside reality perception, we develop a formal model of attitude attribution.

A Model of Attitude Attribution

Attitude attribution depends on a person's own beliefs, plus his or her likes and dislikes. In this sense it is the interconnections of three variables—attribution, belief, and affect—that we should like to understand. But if there are two causal factors, there are more than two causal processes. Attributions may reflect reality perception, or guessing, or a calculus, among other possibilities. To capture the interplay of these alternative causal processes, a formal model of attitude attribution is required. In addition, it is necessary to develop a model that goes beyond the notion that people simply place groups (or candidates) where they actually stand and that they combine this placement with their own position to calculate their utility or feelings for the group.[6] More is needed than simply adjusting for the different ways that people use seven-point scales (as in Aldrich & McKelvey, 1977). A model of attitude attribution must take into account the interrelationships among a person's own beliefs about an issue, his or her feelings toward a group, and the actual position of the group. Models of this sort are not found in the rational choice literature (e.g., Riker & Ordeshook, 1973) or in works on psychometric scaling (e.g., Torgerson, 1958). Instead, one must look to the research on psychological balance and consistency (e.g., Abelson et al., 1968). Unfortunately, although rich in qualitative findings, this literature is weak in formal descriptions of psychological balance (but see Heider, 1958; Osgood & Tannenbaum, 1955; and Rosenberg & Abelson, 1960). The following model tries to remain faithful to the qualitative results of this research while providing a quantitative basis for fitting the NES data.

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[6] The entire spatial modeling tradition beginning with Downs (1957) through Davis, Hinich, and Ordeshook (1970) and continuing with Enelow and Hinich (1984) has assumed that people could place candidates (and by

Table 6. Average Thermometer Score by Ideology and Education (1976)

	Education					
	0.8 Grades	9-11 Grades	High school diploma	1-3 years college	4 or more years college	Total
Average Feelings toward Liberals						
Extreme liberal and liberal	75.4 (13)[a]	60.9 (19)	68.0 (37)	71.6 (33)	74.2 (44)	70.4 (146)
Slightly liberal	68.3 (9)	60.6 (9)	58.5 (50)	61.3 (38)	60.4 (41)	60.5 (147)
Moderate	55.9 (30)	51.1 (45)	52.1 (169)	53.3 (104)	53.3 (64)	52.8 (412)
Slightly conservative	54.2 (13)	46.1 (18)	48.9 (82)	49.8 (44)	44.7 (77)	47.8 (234)
Conservative and extreme conservative	49.2 (26)	41.9 (21)	36.7 (89)	36.2 (50)	32.1 (60)	37.3 (246)
"No Thought"	55.1 (70)	53.4 (77)	52.5 (141)	54.0 (120)	52.2 (9)	53.4 (317)
Total	56.6 (161)	51.9 (189)	50.9 (568)	53.0 (289)	50.8 (295)	52.0 (1504)
Average Feelings toward Conservatives						
Extreme liberal and liberal	69.6 (14)	54.7 (19)	50.1 (36)	42.9 (33)	38.2 (44)	47.4 (146)
Slightly liberal	55.0 (8)	45.0 (10)	52.2 (50)	49.6 (38)	47.0 (41)	49.7 (147)
Moderate	62.3 (33)	58.5 (46)	57.8 (181)	57.5 (106)	58.1 (64)	58.2 (430)
Slightly conservative	71.7 (13)	67.4 (19)	62.4 (82)	66.4 (45)	62.3 (77)	64.0 (236)
Conservative and extreme conservative	65.6 (29)	68.3 (23)	70.7 (92)	73.4 (50)	74.0 (60)	71.2 (254)
"No Thought"	53.8 (79)	58.9 (81)	56.4 (145)	61.0 (20)	51.3 (8)	56.5 (333)
Total	60.0 (176)	59.6 (198)	59.2 (586)	59.2 (292)	57.7 (294)	59.1 (1550)

[a] Sample sizes are in parentheses.

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implication, other political objects, such as social or political groups) in an n-dimensional space. Empirical research in this tradition, such as the work of Page (1978), follows this lead although it often assumes that candidate positions are ambiguous or uncertain. (Also see Page, 1977, for a critique of spatial models.)

We assume that individuals consider (we leave unspecified the exact mental process by which this consideration occurs) the "true" position of each group T_{ij}. This true position may vary from person to person (hence the subscript i), for reasons discussed in a moment, as well as from group to group (j). If people just cared about accuracy, then they would try to minimize the discrepancy or squared distance between where they place the group P_{ij} and where the group actually stands.[7] They would try to minimize their inaccuracy:

$$a(P_{ij} - T_{ij})^2, \ a \text{ greater than zero.} \qquad (1)$$

The parameter a in this equation and similar parameters d and e introduced below represent the weights people place on minimizing each type of imbalance or inconsistency. The obvious solution to minimizing inaccuracy as described by equation (1) is to set $P_{ij} = T_{ij}$ which would be, from the individual's perspective, an *accurate* placement of the group.

But what is the value of T_{ij} in this case? If the individual is fully informed, T_{ij} will probably reflect some common "baseline" image of the group, T_j; if the person is not very knowledgeable and has no image of the group, then T_{ij} will be nothing more than a guess. On seven-point scales, guessers should gravitate to the middle of the scale, or zero if the scale has been centered by subtracting four from all responses, as we shall assume throughout the rest of this discussion. Hence, the value of T_{ij} would be expressed by the following equation:

$$T_{ij} = F_i T_j + (1 - F_i) 0 = F_i T_j \qquad (2)$$

where F_i is the probability of not guessing.

Accuracy is only one concern; consistency is another. There is, it is generally acknowledged, a strain to consistency in belief systems. But with respect to what, precisely, is consistency maximized? It is reasonable, as a starting point, to suggest that people will want to bring their placement of a group into line with their own beliefs B_i and their feelings A_{ij} about the group. At the same time, they will want to bring their feelings about the group into line with their perceptions of it; that is, there might be a simultaneous relationship between feelings about a group and perceptions of it.[8]

First of all, consider how people might try to bring perceptions in line with feelings. Many individuals might want to believe that the world, or at least some groups, agree with them, so that they will strive to minimize the following quantity:

$$d(P_{ij} - B_i)^2, \ d \text{ greater than zero.} \qquad (3)$$

This can be done by setting P_{ij} equal to B_i. Of course, other individuals may want to believe that certain groups disagree with them so they will endeavor to maximize this quantity, or more simply, they will set d to some negative quantity and they will minimize equation (3). In this case, they will choose P_{ij} to be as far away as possible from B_i.

This way of putting things has a point of ambiguity worth clarifying. Equation (3) assumes that people tend to bring the views of groups they like into line with their own—an assumption, however, that can mean two rather different things. It may refer to a systematic tendency to perceive other groups' views as similar to one's own—the so-called "false-consensus" effect (Ross, Greene, & House, 1977)—or alternatively, to a tendency to tailor perceptions about groups to one's feelings about them. The following linear form, specifying the quantity d, provides a suitable way to distinguish these two ways that belief may be of importance:

$$d = b + cA_{ij}. \qquad (4)$$

In this equation, A_{ij} is positive if one likes a group and negative if one dislikes it. The constant b, if it is positive, represents a false consensus effect, and c, hypothesized to be positive, represents a more focussed, or partisan, effect.

As people try to reconcile their perceptions with the feelings they express to the interviewer, they will also try to reconcile these expressed feelings with their true feelings A_{ij}^*. This requires minimizing the following quantity:

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[7] Equation (1) and subsequent equations use the square of the Euclidian distance as a measure of discrepancy between two points in people's psychological space. There is a long tradition of doing this in both psychometrics (see, e.g., Coombs, 1964) and political science (e.g., Davis, Hinich, & Ordeshook, 1970). This equation also assumes that people minimize a function. For a defense of minimum and maximum principles in the social sciences, see Samuelson (1972).

[8] Most of the social psychological research on perceptions simply assumes that feelings cause perceptions (see, for example, Kinder, 1978; and Granberg, Brent, & King, 1981), but the rational choice literature assumes just the opposite. Consequently, our model will test for the direction of causality between feelings and attributions. One could go one step further and assume that people's own beliefs are also endogenous, and we are sympathetic with this notion, but beliefs are probably more solidly grounded than feelings or attributions; rational choice theorists would probably assume that beliefs are exogenous with respect to affect and attributions; and our perceptions or attributions equation is still identified even if beliefs are endogenous (see note 10).

$$e(A_{ij} - A_{ij}^*)^2, \; e \text{ greater than zero.} \qquad (5)$$

The quantity A_{ij}^* is, by definition, unobserved, but it seems reasonable to suppose that it will be a function of the observable characteristics, X_{it}, of the individual:[9]

$$A_{ij}^* = \Sigma_t X_{it} \alpha_{tj}. \qquad (6)$$

Combining equations (1) through (6), we obtain the following choice problem:

$$\begin{aligned} &\text{Minimize } \{a(P_{ij} - F_i T_j)^2 \\ &\quad + (b + c A_{ij})(P_{ij} - B_i)^2 \\ &\quad + e(A_{ij} - \Sigma_t X_{it} \alpha_{tj})^2\} \qquad (7) \\ &\quad \text{with respect to } P_{ij} \text{ and } A_{ij}. \end{aligned}$$

In words, this amounts to choosing P_{ij} in order to have an optimal tradeoff between the inaccuracy in the first term and one's desire for agreement or disagreement with the group in the second term and simultaneously choosing A_{ij} in order to have an optimal tradeoff between the desire for agreement or disagreement in the second term and the need to be consistent with one's true feelings in the last term. The solution to this minimization (or balancing) problem can be found by taking the partial derivatives of equation (7) with respect to P_{ij} and A_{ij} and setting the results equal to zero. After some algebraic manipulation this yields the following system of equations:

$$\begin{aligned} P_{ij} &= [a F_i T_j \\ &\quad + (b + c A_{ij}) B_i]/(a + b + c A_{ij}) \\ A_{ij} &= -(c/2e)\,[P_{ij} - B_i]^2 + \Sigma_t X_{it} \alpha_{tj}. \qquad (8) \end{aligned}$$

Of course, if this is to be a true minimum, the diagonal elements of the Hessian (the matrix of second partial derivatives) must be greater than zero, and the determinant of the Hessian must be greater than or equal to zero (see Intriligator, 1971). This imposes the following constraints on the parameters of this model:

$$\begin{aligned} &a + b + c A_{ij} > 0; \quad e > 0 \\ &\text{and } e(a + b + c A_{ij}) - c^2 (P_{ij} - B_i)^2 \geq 0. \qquad (9) \end{aligned}$$

If A_{ij} is standardized to run between -1 and +1 and if the parameter a is set to 1 to determine the scale, then a necessary condition for a minimum, given the possibility that A_{ij} can equal -1 is that $(1 + b - c)$ be greater than 0.

Other models are also possible. If it is assumed that all perceptions about a group and its opposite (e.g., liberals and conservatives) are made simultaneously, then the choice problem has to be expanded by adding new terms for each type of perception and new terms for affect toward the opposite group. In Appendix 1 we show that the solution to this minimization problem is different for the feelings equation, but exactly the same for the perception equation. As a result, we can go ahead and estimate the perceptions equation with nonlinear two-stage least squares (Kelejian & Oates, 1979, chap. 8) without obtaining biased estimates.[10] We will also report the results of the feelings equation because we have found that our estimates do not change very much when we estimate the entire system of equations.

Estimating and Testing the Model

Some decisions must be made about operationalizing P_{ij}, B_i, and A_{ij}. For ease of interpretation, the first two are centered by subtracting four from all values. Affect, at least initially, is indexed as the difference between feeling toward group j and feeling toward its polar opposite, j', divided by 100. This approach provides a measure of affect that runs from -1 to +1, with a natural zero point that represents the case where the two affect scores are equal.[11] Finally, the likelihood of not guessing, F_i, is represented by a linear function of knowledge about public affairs:

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[9] These true feelings may also vary with the true position of the group, but this effect will be captured in the variation of the coefficients α_{tj}. True feelings may represent the residue of cognitions as an individual repeatedly encounters information about a group, makes judgments about the group from this information, and then forgets the facts on which the judgment was based. How often, for example, do we forget the details of a book while remembering that we liked or disliked it? Feelings may also represent the result of social influence processes in which affect—possibly based upon cognitive judgments by opinion leaders—is communicated throughout a social network. Each of these mechanisms suggests that feelings are based upon prior ratiocination, but that the results of these cognitive processes are economically stored in our feelings.

[10] If beliefs are endogenous and they enter equation (7) as the square of the difference between observed and true beliefs (i.e., in the same way as feelings in equation (5)), then it is easy to see that the partial derivative of the resulting minimand with respect to perceptions will be identical to that obtained for equation (7). Consequently, the perceptions equation will still be identified and identical to equation (8).

[11] The values of B_i and P_{ij} are also assumed to form interpersonally comparable interval scales, and the

$$F_i = g + hK_i. \qquad (10)$$

In this equation, h is assumed to have a positive sign because guessing decreases as knowledge increases.

None of the questions on the 1976 National Election Study is a really good measure of political knowledge. Consequently, we developed a proxy of knowledge by summing up answers to a series of questions about whether the respondent followed public affairs (a four-point scale starting at zero for "hardly at all" to "most of the time"), heard about the campaign on radio, read about the campaign in magazines, watched the campaign on television, or read a daily newspaper (all scored zero for no and one for yes). This eight-point scale is used to represent knowledge.

The usual assumption of an additive error term is made so that the perceptions equation in equation system (8) becomes:

$$\begin{aligned} P_{ij} = {} & [g\,T_j + h\,T_jK_i \\ & + (b + c\,A_{ij})\,B_i]/(a + b + c\,A_{ij}) \\ & + \text{error}. \end{aligned} \qquad (11)$$

In this version, only gT_j and hT_j can be estimated so that an additional identification condition is necessary to obtain the three parameters g, h, and T_j. We have assumed that 10% of the population guesses, $E[F] = .90$, at the average knowledge level $E[K]$.[12] This figure is consistent with the results obtained from the technique described in Appendix 2. This method assumes that the observed responses on a question are the net result of two processes: a fraction of the respondents $(1-\alpha)$ actually have an opinion and are drawn from parametric distributions with location and spread parameters which vary across issues, and the remaining respondents (α) only guess by choosing a response from a distribution with location and spread parameters that are fixed across issues. By making some assumptions about the shape of each distribution, this method yields estimates of the parameters of each distribution and the fraction of guessers. For a sample of 50 placements (the current limit of the computer program), the fraction of guessers is estimated to be approximately 8.5% with a standard error of a few percentage points. We have rounded this off to 10% in our calculations.

The system of equations (8) should be estimated by a method such as nonlinear two-stage least squares, but it is instructive, and of some practical use, to know if the less complicated (and less expensive) method of nonlinear ordinary least squares yields the same results. Tables 7 and 8, therefore, summarize the results of both methods applied to the equations for liberal and conservative and Democratic and Republican placements on the government guaranteed jobs scale. For the X_{it} variables we chose union membership (one if a member, zero otherwise), race (zero if black, one otherwise), income (the 20-point NES scale), and education (highest grade level completed divided by 17 to create a scale running from zero to one). These variables have the great advantage of being truly exogenous to the model, and they were used along with B_i, B_i^2 and the knowledge variable described earlier as instruments for two-stage least-squares estimation.

A quick glance at Tables 7 and 8 indicates that the crucial coefficients of the endogenous variables, c and $c/2e$, are both statistically significant whether nonlinear ordinary or two-stage least squares is used to estimate the models. In fact, it looks as if the magnitudes of these coefficients, especially the magnitude of c, is generally bigger for the two-stage estimates. This suggests, and many other runs confirm, that we can treat the nonlinear ordinary least-squares estimates as conservative measures of these important coefficients. As a result, the empirical work not reported in Tables 7 and 8 but referred to below is based upon nonlinear ordinary least-squares estimates.[13]

The final column of Tables 7 and 8 reports one measure of overall success: the multiple Rs, depending on the issue and group, range from .27 to .35 for the perceptions equation, and from .38 to .48 for the feelings equation. The average multiple correlation for nonlinear OLS estimates of the perceptions equation across all 1976 issues for liberals and conservatives, Democrats and Republicans, and blacks and whites is .32. This is encouraging, given the nature of the data. A less

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values of A_{ij} are assumed to form an interpersonally comparable ratio level scale.

[12] With this assumption, equation (10) can be written as:

$$E[F] = .9 = g + h\,E(K).$$

From estimates of gT_j, hT_j, and $E[K]$, an estimate of the quantity z, described by the following equation, can be obtained and used to estimate T_j as $(z/.9)$:

$$z = g\,T_j + h\,T_j\,E[K] = T_j\,(g + h\,E[k]).$$

[13] This work is also based upon using education instead of attention to the media as the proxy for knowledge. For theoretical reasons we prefer the former, but the empirical results seem quite similar.

Table 7. Attributed Positions of on Government Guaranteed Jobs (1976)

Attributed Positions	N	Estimation Method	Estimated Quantities				Derived		Multiple R[b]
			gT_j	hT_j	b	c	T_j	h	
Liberals	966	N2SLS	.192	–.217	.083	.464	–1.040	.209	
			(.195)[a]	(.040)	(.066)	(.260)			
		NOLS	.163	–.208	.083	.493	–1.021	.204	.349
			(.187)	(.036)	(.035)	(.051)			
Conservatives	966	N2SLS	–.150	.205	.172	.943	1.018	.201	
			(.200)	(.037)	(.055)	(.182)			
		NOLS	–.183	.219	.109	.562	1.062	.206	.305
			(.183)	(.034)	(.034)	(.082)			
Democrats	1136	N2SLS	–.750	–.089	.245	1.394	–1.348	.066	
			(.528)	(.092)	(.124)	(.223)			
		NOLS	–.707	–.106	.128	.615	–1.398	.076	.319
			(.156)	(.030)	(.029)	(.058)			
Republicans	1136	N2SLS	–.338	.141	.157	.999	.439	.321	
			(.171)	(.031)	(.048)	(.102)			
		NOLS	–.115	.122	.034	.517	.577	.214	.272
			(.137)	(.026)	(.027)	(.060)			

[a] Standard errors are in parentheses.

[b] Multiple *R*s are only available for NOLS.

Table 8. Feelings toward Left and Right Groups on Government Guaranteed Jobs (1976)

Feelings toward	N	Estimation Method	$c/2e$	Intercept	Union	Race	Income	Education	Multiple R[b]
Liberals	966	N2SLS	.00838	.130	.0669	–.221	–.00686	.198	
			(.0014)[a]	(.049)	(.0211)	(.033)	(.00182)	(.059)	
		NOLS	.0107	.137	.0606	–.211	–.00632	.198	.477
			(.0008)	(.049)	(.0208)	(.033)	(.00180)	(.059)	
Conservatives	966	N2SLS	.0085	–.0231	–.0746	.2175	.0080	–.185	
			(.0022)	(.0548)	(.0221)	(.0356)	(.0019)	(.062)	
		NOLS	.0088	–.0206	–.0742	.216	.0080	–.185	.385
			(.0011)	(.0519)	(.0218)	(.035)	(.0019)	(.062)	
Democrats	1136	N2SLS	.0056	.421	.0738	–.237	.0066	–.0713	
			(.0011)	(.036)	(.0161)	(.025)	(.0014)	(.0445)	
		NOLS	.00646	.423	(.0707)	–.233	–.00646	–.0707	.473
			(.00068)	(.036)	(.0158)	(.025)	(.00136)	(.0444)	
Republicans	1136	N2SLS	.00308	–.361	–.0894	.244	.00687	.0668	
			(.00143)	(.043)	.0160	(.027)	(.00139)	(.0455)	
		NOLS	.00613	–.314	–.0862	.224	.00651	.0584	.453
			(.00079)	(.038)	(.0159)	(.0253)	(.00138)	(.0450)	

[a] Standard errors are in parentheses.

[b] Multiple *R*s are only available for NOLS.

obvious, and stronger, confirmation is this: our theory about how perceptions are formed, summarized in the first equation in system (8), predicts that there is no intercept term. As a matter of common experience, however, a linear regression without an intercept term almost always fits a set of data worse than one with an intercept term; one would usually expect the same to be true with a nonlinear equation. Yet, when four different issues were run with an intercept term, the result was changing signs and an average t-statistic for the intercept of only 1.455. Another test involved comparing some of the nonlinear equations in Table 8 with linear equations with an intercept, a belief, an affect, and an interaction term. Despite their additional free parameter, the linear equations did no better than the nonlinear equations.

But our real interest is understanding the process by which people arrive at an estimate, or impression, of what liberals and conservatives stand for on major issues. Perhaps the first term to note is T_j in Table 7. It indicates the extent to which estimates are reality oriented—that is, if people correctly place liberals on the left and conservatives on the right. By this standard, preferences are indeed reality oriented in every instance. (It is worth observing in passing that this holds not only for placement of liberals and conservatives; it is true as well for estimates of the parties and, what is more, for estimates of blacks and whites.)

The values of hT_j and h are also of interest. Except for one case, the first are all statistically significant in Table 7, and the derived values of h always have the proper sign. Moreover, the pattern for all 34 equations for 1976 is similar. This result provides confirmation at the individual level of our earlier finding that some respondents guess when they are asked to place groups on the seven-point scales.

This is not to say that people's impression of what a group stands for on a particular issue is unconnected with what they themselves think about that issue and how they feel toward those groups. Indeed, the results of Table 7 give a remarkably explicit and definite idea of what the connections are. Thus, one possibility is that estimates of what people think others think are systematically biased —the so-called false consensus effect—such that people are inclined to think that most groups agree with them more than they in fact do. Examining the coefficient b, we see that it is never negative, indicating the absence of any significant tendency to emphasize disagreement. Looking at Table 9, which summarizes a large number of nonlinear ordinary least squares estimates, the value of b is always positive, or nearly so,[14] with an average value of .176 across all of the 1976 issues and groups and an average standard error of only .032 (for an average t-statistic of 5.43). This implies that there is a tendency for people to draw the world toward themselves.

But people also understand the world of politics in terms of their feelings, and this is reflected in the value of c. The value of this parameter is not only statistically significant in the expected direction in all of the equations presented in Table 7, it is significant in every one of the 34 equations for 1976 in Table 9. Moreover, the average value of c across all the issues is .548 with an average standard error of .079 for an average t-statistic of 6.96, and the first condition in equation system (9) is met in every case. Two points deserve specific mention. First, it is the interaction of affect and belief, and not the independent effect of either, that is telling. Second, it is the combination of feelings toward liberals and toward conservatives that matters. The first point is straightforward, the second less so; and so we should like to say a further word about it.

Various operationalizations of affect have been tried, and the functional form of the first equation in system (8) varied, allowing assessment of an array of rival formulations. For example, it might be supposed that what matters is the absolute level of one's feeling toward the group being placed on the scale, and that one's feeling toward the opposite group is irrelevant. Columns three and four of Table 9 present the nonlinear ordinary least squares results when the absolute levels of affect (divided by 100 so that they run from zero to one) toward the two groups are included separately in an equation like (11). The result is clearcut, the coefficient of affect toward the opposite group, c_2, averages approximately −.32 with an average standard deviation of about .13. This compares favorably with the average coefficient of affect toward the group being placed, c_1, of .70 with an average standard deviation of approximately .10. As political—and, above all, ideological—choices tend to be two-sided (e.g., liberal vs. conservative), affect toward polar groups is especially effective in estimating whether these groups are on one side or the other to the extent that it involves a preference for one of the sides, as against the other.[15]

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[14] The only exception that we have managed to uncover, ransacking all the combinations of issues and groups, is the position of Republicans on busing. In addition, b is insignificantly positive in four other cases.

[15] Although the results in Table 9 also suggest that feelings toward the group being placed are somewhat more important than feelings toward the opposite group, for two reasons we have chosen to use only the difference in affect in the equations reported in Tables 7 and 8. First, taking the difference provides a natural

Yet affect might be thought less important if it could be shown that the impact of perceptions on affect was markedly greater than the impact of affect on perceptions. The relative importance of these two effects can be determined by comparing the derivatives $\partial P_{ij}/\partial A_{ij}$ for the perceptions equation and $\partial A_{ij}/\partial P_{ij}$ for the feelings equation:

$$\partial P_{ij}/\partial A_{ij} = -c\,[B_i - F_i T_j]/(1 + b + cA_{ij})^2$$

$$\partial A_{ij}/\partial P_{ij} = -2(c/2e)\,[P_{ij} - B_i]. \qquad (12)$$

For simplicity, we evaluate this for a neutral respondent, $A_{ij} = 0$, who does not guess, $F_i = 1$, and we assume a one-point discrepancy in each of the bracketed terms. These terms index the degree to which there is imbalance between either a person's beliefs and the true position of a group or between beliefs and perceptions of the group. If the average values of b and c are taken from the nonlinear ordinary least-squares results reported in Table 9, then b is .176 and c is .548. With these values, a conservative estimate of the absolute impact of a change in affect on perceptions is .40—or a movement of at least 27% in the typical standard deviation (1.5) of these perceptions in the sample. This figure is nearly doubled when the larger estimates for b and c from nonlinear two-stage least squares are used. If the larger estimates from this method are used for calculating the average value of $(c/2e)$ as .01, then a liberal value for the absolute impact of a change in perceptions on affect is .02—or a movement of no more than 8% of the typical standard deviation (.25) of this affect measure.

The impact of affect on perceptions, this suggests, is at least three times larger than the impact of perceptions on affect, and possibly as much as five or ten times larger.

Conclusions

It is only prudent to wonder if citizens have genuine opinions on issues involving abstract or ideological preferences (such as the role of government in housing) as opposed to doorstep concerns (such as busing) (Converse, 1970). If it is difficult to credit that citizens know what they themselves believe about such issues, surely it is harder still to credit the idea that they know what others believe about them. This skepticism is understandable. Certainly, many in the mass public have only a minimal knowledge about politics and hold many ideas about it that are only minimally interconnected. All the same, the general public is remarkably accurate at figuring out the issue positions of groups, including liberals and conservatives, who would hardly seem to be highly salient to the general public.

To be sure, the notion of accurately attributing attitudes raises complex issues. The fundamental problem is not the absence of standards of accuracy. Instead it is a plethora of them. Is the liberal position the position held by most liberals? Alternatively, is it the position held by the most liberal? Is the Democratic position the one reflected in the views of Democrats? Or is it the one embodied, in some sense, in the party itself? Is the position of blacks the position characteristic of most blacks? Or is it the one held by black activists, that is, by blacks who characterize blacks for whites. These are complex questions, but whatever their ultimate answer, we have seen that a substantial part of the mass public can correctly locate the positions of key groups as either on the left or on the right on a number of major issues. How is this possible?

Much of the answer, it seems to us, turns round the role of groups in organizing political thinking. This is, of course, far from an original hypothesis: it was at the center of the so-called sociological approach of the Columbia school (e.g., Berelson, Lazarsfeld, & McPhee, 1954) and, although less central, nonetheless consequential in the so-called psychological approach of the Michigan school (Campbell et al., 1960). But it is not enough to say that relying on groups is a device for economizing on information, a way to get round the minimal knowledge that many citizens have about politics. Certainly it is not reasonable to suggest that groups, characteristically, organize the political thinking of mass publics by serving as reference groups, for there are many groups, and many of them (like liberals and conservatives) are not especially salient. How, then, can citizens make sense of groups—that is, know which is relevant to which issue and which stands for what—without having to know a great deal about them? Surely it is no more plausible to suggest that the average citizen monitors closely the political positions of an array of groups than to suggest that he or she attends carefully to other aspects of politics.

A portion of the mass public—not surprisingly, the best educated portion—is capable of reality perception, of attributing attitudes to a group on the basis of knowledge of the group's true position, but they also accentuate their attributions by taking into account their feelings toward groups.

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zero point for our affect scale. Second, and more important, if feelings toward both groups are included in our model, then it becomes impossible to obtain statistical identification. As a result, we have chosen what is probably, given the results in Table 9, only a slightly misspecified model in order to achieve the benefits of an identified model.

Table 9. Estimates of Attitude Attribution Equations

	NOLS with Guessing		NOLS with Separate Affect Measures		NOLS with Guessing		NOLS with Separate Affect Measures	
	b	c	c_1	c_2	b	c	c_1	c_2
	Liberals				Conservatives			
Government-guaranteed jobs	.097 (.035)[a]	.538 (.049)	.701 (.103)	−.331 (.137)	.116 (.034)	.510 (.085)	.599 (.191)	−.478 (.202)
Rights of accused	.255 (.037)	.586 (.059)	.736 (.108)	−.409 (.135)	.288 (.042)	.747 (.101)	.981 (.152)	−.477 (.174)
	Blacks				Whites			
Busing	.375 (.065)	.584 (.084)	.701 (.103)	−.331 (.137)	.340 (.039)	.610 (.157)	.599 (.191)	−.478 (.202)
Minority aid	.094 (.024)	.281 (.043)	.308 (.054)	−.208 (.066)	.311 (.037)	.892 (.116)	1.295 (.135)	−.504 (.145)
Rights of accused	.192 (.031)	.424 (.063)	.619 (.081)	.025 (.100)	.462 (.048)	.675 (.194)	1.100 (.221)	.128 (.242)
Medical insurance	.151 (.021)	.248 (.049)	.267 (.059)	−.195 (.069)	.538 (.044)	1.066 (.185)	1.193 (.240)	−.821 (.252)
Government-guaranteed jobs	.142 (.030)	.408 (.052)	.412 (.063)	−.319 (.081)	.299 (.036)	.563 (.147)	.879 (.172)	−.187 (.182)
	Democrats				Republicans			
Government-guaranteed jobs (pre)	.118 (.028)	.523 (.055)	.532 (.084)	−.472 (.095)	.019 (.025)	.485 (.055)	.517 (.082)	−.421 (.1081)
Rights of accused	.340 (.037)	.692 (.079)	.981 (.121)	−.284 (.146)	.255 (.036)	.780 (.064)	.910 (.099)	−.560 (.138)
Busing	.098 (.035)	.536 (.049)	.546 (.074)	−.544 (.088)	−.041 (.028)	.384 (.056)	.343 (.078)	−.457 (.094)
Minority aid	.165 (.028)	.530 (.059)	.658 (.091)	−.303 (.107)	.026 (.026)	.536 (.056)	.719 (.076)	−.257 (.094)
Medical insurance	.056 (.023)	.455 (.056)	.619 (.081)	−.196 (.090)	.006 (.023)	.415 (.053)	.478 (.078)	−.248 (.094)
Government-guaranteed jobs (post)	.125 (.031)	.555 (.055)	.653 (.082)	−.370 (.099)	.004 (.025)	.538 (.051)	.623 (.070)	−.412 (.096)
Urban unrest	.266 (.034)	.606 (.090)	.767 (.135)	−.200 (.155)	.191 (.032)	.719 (.057)	.910 (.082)	−.374 (.102)
Marijuana	.179 (.027)	.588 (.079)	1.035 (.096)	−.117 (.106)	.101 (.025)	.298 (.076)	.569 (.096)	.015 (.097)
Tax rate	.205 (.030)	.658 (.066)	.763 (.104)	−.504 (.134)	.102 (.026)	.472 (.063)	.607 (.088)	−.258 (.103)
Women	.220 (.030)	.239 (.111)	.653 (.134)	.337 (.146)	.184 (.032)	.553 (.076)	.807 (.107)	−.207 (.115)

b: "False consensus effect."
c: Coefficient of feelings towards group being placed minus feelings towards opposite group.
c_1: Coefficient of feelings towards group being placed.
c_2: Coefficient of feelings towards opposite group.
[a] Standard errors are in parentheses.

Others must rely more heavily on an affective calculus, or likability heuristic, and to the extent they rely on this calculus, they can be remarkably accurate in figuring out the issue position of strategic groups in politics. It would seem worthwhile, therefore, to consider the properties of this likability heuristic and thereby perhaps to throw light on just how groups organize the political thinking of mass publics.

The likability heuristic has two components—first, a person's own beliefs on an issue, and second, the person's feelings toward the pair of opposing groups whose issue positions he or she is estimating. This heuristic can be an effective calculus, in the same sense that incrementalism in budgeting can be: in the one case, a person need only take last year's budget and add some amount to it; in the other, a person need only take where he or she stands, weighted by his or her feelings toward a pair of groups. In short, both are intellectual shortcuts, ways of effectively simplifying questions too complex to address in their own terms. But how, exactly, does the likability heuristic work? Why does it yield accurate estimates of what others believe?

There is, of course, no necessity that attributions be accurate. Indeed, often attributions are not accurate, as research on "false consensus" (cf. Ross, Greene, & House, 1977) and on candidate perception (cf. Brent & Granberg, 1982; Granberg & Brent, 1974; and Page & Jones, 1979) testifies.[16] There is, it seems, a strong tendency in the attribution of attitudes to suppose that others agree with oneself. Nor should this be surprising, considering that this effect is observed, characteristically, when people are asked about the views of "others like yourself" or "other people in this part of the country." Similarly, false consensus shapes the perception of candidates, thanks in part to the efforts of candidates to blur their stands on issues and thereby invite agreement from across a spectrum of positions (Page, 1978). In short, people are likely to exaggerate how much "others"—defined either positively or vaguely or both—agree with them.

Why, then, are many in the mass public able to attribute accurately attitudes to Democrats and Republicans, to blacks and whites, even to liberals and conservatives? Liberals and conservatives (and Democrats and Republicans) have, and emphasize, political identities; identities, moreover, that have been developed in contradistinction to one another (liberals vs. conservatives, Democrats vs. Republicans). Because they are competitors there are incentives—certainly, there is permission—for a person who likes liberals to dislike conservatives, and the other way around. What allows citizens to simplify political calculations efficiently is this two-sided, "us vs. them" character of politics; the more attached they are to their side—and the more opposed they are to the other—the more they appreciate the differences between the issue positions of the two sides. What counts, then, is not how people feel toward groups, one by one; rather it is how they feel toward *pairs* of opposing groups.

Admittedly, this is not the usual way to think of the relation between rationality and affect (or likes and dislikes). Customarily, the two are supposed to be opposed to each other, as though one obstacle to making sense of politics is having intense likes and dislikes. Our findings, however, suggest just the reverse. The person who has the least success in figuring out what the other side stands for is the one who does not dislike them; to understand that the other side really does stand for policies at odds with your own, you should dislike them. More fundamentally, taking account of your likes and dislikes helps ensure the accuracy of attitude attribution to the extent it accentuates the difference between your side and the other. Thus, a liberal who likes his side and very much dislikes the other tends to emphasize *both* how conservative conservatives are and how liberal liberals are. Otherwise, the tendency to presume agreement (false consensus) would get the upper hand, if not in a person's perception of the other side, then in his perception of his own.

The notion of affective consistency is potentially a fruitful one, or so it seems to us (Sniderman & Tetlock, in press). It seems, without it, hard to understand just what holds mass belief systems together, what serves as their glue. The mass public surely does not command much abstract knowledge of politics and as a rule does not even pay much attention to it. It seems implausible, therefore, to suppose that the general public is able, or at any rate willing, to assemble complex cognitive hierarchies of political ideas. In contrast, likes and dislikes are easy to form and, even more important, easy to remember. Accordingly, affect can be a quite efficient way of encoding and storing what is after all the most vital political information: who and what one is for or against.

There should be no need of a lengthy warning on the dangers of reifying affect or of laying out a false choice between affective and cognitive consistency. It should suffice to observe that affect itself may be the residue of long biographies of cognitive transactions. Nonetheless, likes and dislikes can provide a certain cement to mass belief systems, and so supply a foundation, not only for estimating the preferences of others, but also for inferring one's own (Chubb, Hagen, & Sniderman, 1984; Sniderman, Brody, & Kuklinski, 1984).

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[16] Our finding of accuracy, however, is consistent with the pioneering analysis of *Voting* (Berelson et al., 1954), which focused (as we did) on popular perceptions of politically strategic groups.

However that may be, the mass public is remarkably accurate in attributing positions to strategic groups on major issues. Partly this is a matter of reality perception, of having a knowledge of the group's actual position, but partly it is the result of an intellectual shortcut, a likability heuristic, which allows many in the mass public to figure out who stands on the left, and who on the right, without necessarily knowing a lot about politics or ideology. Accordingly, it may be a mistake to suppose that ideological reasoning, in this sense, is altogether outside the reach of many of the mass public; a mistake because ideological reasoning, at its core, involves an understanding of who wants what, and many citizens can figure this out, thanks to the role of groups in organizing their political thinking. No doubt this is a partial understanding of ideology; all the same, it is an understanding of a particularly important part of it.

Appendix 1
An Expanded Choice Problem

Assume that people choose all their perceptions about and feelings for a group at the same time. To describe this situation, we add the subscript m, for issues, to the perceptions, P_{ijm}, true positions, T_{jm}, beliefs, B_{im}, and all of the constants in equation (7). In addition, we use primes to represent the opposite group so that we have P_{ijm}', T_{jm}', A_{ij}', $A_{ij}'^{*}$, and primes on all of the constants. The minimand is then:

$$\begin{aligned} Q = {} & \Sigma_m a_m (P_{ijm} - F_i T_{jm})^2 \\ & + \Sigma_m a_m' (P_{ijm}' - F_i T_{jm}')^2 \\ & + \Sigma_m (b_m + c_m A_{ij}) (P_{ijm} - B_{im})^2 \\ & + \Sigma_m (b_m' + c_m' A_{ij}') (P_{ijm}' - B_{im}')^2 \\ & + \Sigma_m e_m (A_{im} - A_{im}^{*}(^2 \\ & + \Sigma_m e'_m (A_{im}' - A_{im}'^{*})^2. \end{aligned} \tag{13}$$

When this quantity is minimized, the resulting equations are:

$$\begin{aligned} P_{ijm} = {} & [a_m F_i T_{jm} \\ & + (b_m c_m A_{ij}) B_i]/(a_m + b_m + c_m A_{ij}) \end{aligned}$$

$$A_{ij} = -\Sigma_m (c_m/2em) (P_{ijm} - B_i)^2 + A_{ij}^{*} \tag{14}$$

with similar equations for the primed quantities. Notice that the perceptions equation is exactly the same as in equation (8), and the feelings equation is similar except that the first term on the right-hand side is now a summation over all issues for the group, which suggests that the estimates of the feelings equations reported in the text will be biased unless these additional terms are uncorrelated with those terms in equation (8).

Appendix 2
The Fraction of Guessers

It is not common to treat guessing as a bias, perhaps from a reluctance to second guess respondents. But given the demonstrated limits of mass understanding of political abstractions, it behooves us to consider it. The NES surveys may subtly encourage people who know where they stand on an issue also to tell the interviewer where they think groups stand on this issue, even though they are only guessing, because people may feel that knowing where these groups stand is a necessary concomitant of having their own view, and the survey only asks about the groups after asking individuals to place themselves on the scale. This approach may encourage people to place groups even though they know very little about where the groups stand.

For example, consider the following model. Assume that a large number of people, for example I, are asked to respond to each of M items on a sample survey. (We could index items by both the group, j, and the issue, m, but in this appendix, we simplify matters by just using m.) Further suppose that each item has $K+1$ categories ($k=1, \ldots, K+1$). Assume that for each item, α of the respondents guess when they provide a response.

For the nonguessers, assume that they all share a common underlying dimension and that their response to question m is based upon their value for this unobserved random variable Y_m with respect to some unobserved "category boundaries" X_k which are assumed to be the same across all M questions. These categories are assumed to be such that X_{k+1} is greater than X_k for all k, and it is assumed that X_0 is minus infinity and X_{K+1} is plus infinity. Specifically, a nonguessing person i will respond with the k'th category on the m'th question if Y_{im}, the person's specific value for Y_m, is greater than X_{k-1} but less than X_k. Represent person i's choice of the k'th category on the observed scale by setting q_{ikm} equal to one and q_{ijm} equal to zero for $j \neq k$. Then the probability π_{km} that q_{ikm} equals one for a nonguesser can be expressed as:

$$\begin{aligned}\pi_{km} &= \text{Prob}(q_{ikm} = 1)\\ &= \text{Prob}(X_{k-1} < Y_m < X_k)\\ &= F_m(X_k) - F_m(X_{k-1}) \qquad (15)\end{aligned}$$

where $F_m(.)$ is the cumulative distribution function of Y_m..

Assume that for these nonguessers the F_m can be parameterized by a location parameter, μ_m, a spread parameter, σ_m, and a common measure of kurtosis, γ so that we can write the following:

$$\begin{aligned}V_{km} &= (X_k - \mu_m)/\sigma_m\\ W_{km} &= \text{sgn}(V_{km})\,|V_{km}|^\gamma\\ F_m(X_k) &= G(W_{km}) \qquad (16)\end{aligned}$$

where $G(.)$ is chosen to be some absolutely continuous distribution function. If γ is one and if $G(.)$ is the cumulative normal, then F_m is just a normal distribution with mean μ_m and standard deviation σ_m, but much more general distributions are possible with this formula. One interesting subcase is the class of exponential power distributions discussed by Box and Tiao (1973, pp. 156-159).

Assume that a similar story holds for the guessers except that they have a common distribution across all items with a location parameter μ^*, a spread parameter σ^*, and a kurtosis parameter γ^*. Then the probability π_k^* of a guesser being in category k will be:

$$\begin{aligned}\pi_k^* &= \text{Prob}(q_{ikm} = 1)\\ &= \text{Prob}(X_{k-1} < Y_m < X_k)\\ &= F^*(X_k) - F^*(X_{k-1})\\ &= G^*(W_k) - G^*(W_{k-1}) \qquad (17)\end{aligned}$$

$$\begin{aligned}\text{with: } W_k &= \text{sgn}(V_k)\,|V_k|^{\gamma^*}\\ V_k &= (X_k - \mu^*)/\sigma^*\\ F^*(X_k) &= G^*(W_k)\end{aligned}$$

where $F^*(.)$ is the cumulative distribution function of Y^* for the guessers and where $G^*(.)$ is chosen to be some specific absolutely continuous distribution function.

With this notation, we can write the probability ϕ_{km} that any member of the sample, whether guesser or nonguesser, is in category k of question m as follows:

$$\phi_{km} = \alpha\,\pi_k^* + (1 - \alpha)\,\pi_{km}. \qquad (18)$$

The probabilities ϕ_{km} can be written as a function of the unknown parameters:

α	—the proportion of guessers,
μ_m	—the location parameters for the nonguessers,
μ^*	—the location parameter for the guessers,
σ_m	—the spread parameters for the nonguessers,
σ^*	—the spread parameter for the guessers,
γ	—the kurtosis parameter for the nonguessers,
γ^*	—the kurtosis parameter for the guessers,
X_k	—the common category boundaries,

and we represent these parameters by the vector θ so that $\phi_{km} = \phi_{km}(\theta)$. The observed probabilities in the sample are:

$$p_{km} = \Sigma_i\, q_{ikm}/I \qquad (19)$$

so that once the forms of $G(.)$ and $G^*(.)$ have been chosen, the statistical problem is to choose the unknown parameters θ so as to obtain the best fit between the $\phi_{km}(\theta)$ and the observed p_{km}.

In this situation, minimum chi-square and maximum likelihood are asymptotically equivalent (at least in terms of first order efficiency; see Rao, 1973). Hence we choose to solve the following problem which turns out to be easier to solve than the maximum likelihood estimation problem:

$$\text{Min}\, \Sigma_m\, (\phi_m - p_m)\ \ \Omega^{-1}\,(\phi_m - p_m)' \qquad (20)$$

where we define:

$$\begin{aligned}\phi_m &= (\phi_{1,m}, \ldots, \phi_{k,m}, \ldots, \phi_{K+1,m})\\ p_m &= (p_{1,m}, \ldots, p_{k,m}, \ldots, p_{K+1,m})\\ \Omega &= \text{Var}(p_m).\end{aligned}$$

$\text{Var}(p_m)$ can be further defined in the following terms:

$$\begin{aligned}\text{Var}(p_{km}) &= \phi_{km}\,(1 - \phi_{km})/I, \text{ and}\\ \text{Cov}(p_{km}, p_{lm}) &= -\phi_{km}\,\phi_{lm}/I. \qquad (21)\end{aligned}$$

It can be shown that minimizing equation (20) yields consistent estimates of the parameters. Moreover, these estimates will be asymptotically normal, and this makes it possible to undertake the statistical hypothesis testing described in the text.

A computer program has been written to minimize equation (20) when $G(.)$ and $G^*(.)$ are assumed to be the logistic distribution. This program obtains starting values by assuming that α is zero, that the scale is symmetrical and that γ is one. Then, a series of Gauss-Newton and gradient iterations are used to find the minimum of the generalized least-squares form in equation (20).

Using this method for a sample of 50 placements (the current limit of the computer program), the kurtosis parameter for the distributions of the nonguessers is .987 with a standard error of approximately .01 and the category boundaries when symmetry is imposed are -4.17, -2.48, -1.0, 1.0, 2.48, and 4.17 versus -4.13, -2.45, -1.0, 1.0, 2.52, and 4.18 when symmetry is not imposed (with standard errors approximately .10). Finally, the fraction of guessers is estimated to be approximately 8.5% with a standard error of a few percentage points.

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The Bear Market in Political Socialization and the Costs of Misunderstood Psychological Theories

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Not so long ago, political scientists were enthusiastically proclaiming that political socialization was a growth stock. But interest in the subfield has slackened, and the bull market has turned bearish. This article argues that a central cause of this recent scholarly neglect is a lack of theoretical confidence. Political socialization has been branded as less worthy of study largely because it is difficult to study and to understand in the absence of an explicit psychological model of learning. A strong theoretical rationale must be developed to return the subfield to its deserved place of priority. Such an endeavor is also justified by the new popularity of the Piagetian model, which is inappropriate for understanding political learning because it emphasizes the foremost growth of logical operations and the individual as the prime motivating force. Another model, that of L. S. Vygotsky, is more useful, incorporating many of Piaget's insights without his unrealistic expectations. I outline Vygotsky's cognitive-developmental model, indicate its applicability to the small body of defensible research on the process of political learning, and conclude with a research agenda suggested by the model.

Whatever happened to political socialization? It was not long ago that enthusiastic practitioners noted the burgeoning amount of research and number of researchers in the field, which signaled to them the arrival of a crucial subdiscipline (see, e.g., Dennis, 1968, 1973; Greenstein, 1970; Hyman, 1969; Schonfeld, 1971; Sears, 1975). All of these authors probably would have agreed wholeheartedly with Greenstein's (1970, p. 969) triumphant proclamation, "Political socialization is a growth stock."[1] But just as in real-life stock markets, a boom is often followed by a bust, and political socialization futures are no longer such hot sellers. The confident predictions in the late sixties and early seventies that the golden age of research was imminent have failed to materialize. To be sure, rumors of the demise of political socialization are as "greatly exaggerated" as Mark Twain's reported death. But concerns for the subdiscipline's continued vitality are not.

Empirical indicators of the decline of interest in political socialization are not difficult to find. In 1975, Sears could note that "Research output has increased at a geometric rate, resulting recently in the publication of several texts and readers" (p. 94). Today, of the nine texts and readers mentioned by Sears, six are out of print and have not been replaced by newer volumes; the last socialization text to appear was published in 1978.[2] Similarly, in 1973, Dennis could point to a notable upswing of articles in the field; whereas there had been no articles in the *American Political Science Review* and only eight in three regional journals between 1960 and 1965 inclusive, the next six years saw 12 in the *Review* and 21 in the three others. However, by my calculations for the years 1977 to 1982 inclusive, the *Review* published only seven articles and the same three regional journals only 16—numbers that are yet less impressive when one takes into account the greater quantity of articles published in more recent years.

The bear market in American political socialization research has hit one sector in particular hardest. Of the 23 articles published in the four

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[1] Lest it be thought that Greenstein is alone in becoming carried away by colorful language, I might equally cite Hyman's (1969, p. vi) foreword to the second edition of *Political Socialization*, first published in 1959: "From the perspective of the present, the image of the scientific landscape of the past is bound to be distorted. With this luxuriant scene before our eyes, it is hard to bring back to mind the barren vista of the previous decade."

[2] This is Stacey (1978); Dawson, Prewitt, and Dawson (1977) is a second edition of an earlier text. The marketability of books on political socialization has also been used by publishers as a reason to reject manuscripts in recent years.

journals during those six years, only two concerned subjects younger than high school seniors. Just as contemporary commentators bemoan the "disappearance of childhood" in everyday life (Postman, 1982), so childhood has disappeared in political science as well. And such an omission is significant. Certainly political socialization is more than the study of the incorporation of children into the political world, but we may have difficulty comprehending "adult political socialization," artificially divorced from its necessary base in earlier learning. The current attention to later political learning (or lack thereof) threatens the raison d'etre of political socialization, which, by necessity, must now extend to all aspects of adult political behavior in a variety of social and institutional contexts. Thus, not only is there less research devoted to political socialization, but the work that is pursued is less distinguishable from other lines of study in political science. In the absence of attention to preadult political learning, one can only question the claim of political socialization to represent a separate subdiscipline.

This scholarly lack of interest in political socialization, and particularly children's political learning, might make sense if there remained few pressing questions to answer, but on the contrary, one would have to conclude that political scientists understand *less* about the process of political socialization than they did (or than they thought they did) 10 or 15 years ago. Indeed, it is childhood that is least understood and about which research is least in evidence. The conclusions from the late sixties of the effective socialization of children into system-supporting party identifiers have been undercut by a wealth of methodological and substantive problems. The model of childhood political socialization sketched in the early studies is, by now, highly problematic, yet there are few attempts to rebuild our knowledge—unlike, for example, the field of public opinion, where confusion about just what we know and what we don't know has not discouraged continuing research.

Nor can I attribute the bear market to a new view of political socialization as an insignificant subject. Even revisionists, who wish to deflate, for example, the idea of partisanship as an affective orientation absorbed during childhood from one's parents, cannot contend that party preference has no origins before adulthood (e.g., Fiorina, 1981). Furthermore, some political scientists recognize the theoretical questions faced in political socialization to be central concerns. As Lindblom noted in his presidential address to the 1981 annual meeting of the American Political Science Association:

> Many conventional scholars see political socialization as a lifelong process . . . in which citizens "mature." Socialization helps the citizen to "comprehend" and "evaluate." It gives him "cognitive growth" and "increasing grasp." . . . Such a view slights the possibility that socialization is intellectually confining, is sometimes crippling, may reduce understanding and may obstruct the development of skill in evaluation. But which of these it does, or in what mixture, is as important a question for political science as can be examined. (1982, p. 17)

The scholarly neglect of socialization seems to result instead from a loss of confidence about what it is we are measuring and then what it all means. Part of this lack of confidence is derived from the traditional methods used to study political socialization. Whatever one may think of survey research as a way of tapping adult political behavior, it is now clearly inappropriate for examining children's reactions to politics, at least before mid- to late-adolescence. The descriptions of glowing idealization of political figures from the earliest studies were based on forced-choice answers to sometimes unrealistic questions, often through pencil-and-paper questionnaires which may have seemed more like a test of socially correct views rather than an opportunity to speak one's mind. Children's responses under such conditions have been aptly designated as nonattitudes marked by high instability (Connell & Goot, 1972/73; Vaillancourt, 1973) and as by-products of children's willingness to guess, and to do so in a way marked by social correctness, positivity, and response set (Kolson & Green, 1970). Although the vast bulk of socialization research, based as it is on dubious methods and problematic findings, must thus be set aside, several perspicacious researchers have answered these challenges with highly successful innovations such as semi-structured interviews (e.g., Connell, 1971), semi-projective tests (e.g., Greenstein, 1975), ethological observation (e.g., Barner-Barry, 1977), and hypothetical dilemmas (e.g., Adelson, 1971; Adelson & O'Neil, 1966; Crain & Crain, 1974; Merelman, 1971a), and there are other methods, such as Q-sorts, experiments, and participant observation, whose utility has not yet been exploited to study children's political development.

Nevertheless, the stock response among students of political socialization has been to cast doubt on the abilities of the young subjects, not on the data-gathering instruments themselves.[3]

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[3] The pessimism about being able to chart children's attitudes from children's responses is so great that one distinguished scholar has gone so far as to suggest we'd be better off using the notoriously unreliable recall measures in adult surveys (Weissberg, 1976).

This has been acknowledged by the growing popularity of surveying high school seniors, whose responses to surveys are no less reliable than those given by adults (Schwartz, 1977), and by the tendencies of political socialization scholars to examine even older subjects (e.g., Jennings & Niemi, 1981). In effect, because it is more difficult to examine, the process of political learning in childhood has been tacitly branded as less worthy of study.

This loss of confidence is not then solely methodological; if it were, scholars would undoubtedly have turned to more effective methods to study children than the tools of survey research. Instead, the bear market is also attributable to the absence of a clear rationale for study, based in psychological models of development. The first studies were so preoccupied with presenting a coherent "political theory of political socialization" (Easton & Dennis, 1969) that they sidestepped psychological theories of the learning process, sometimes explicitly. At best, psychological models were assembled in an ad hoc fashion. Critiques (March, 1971; Searing, Schwartz, & Lind, 1973) correctly questioned whether the impact of children's political learning upon subsequent adult political behavior could be so blithely assumed. After all, such a link could not be demonstrated by intuitive notions such as the oft-quoted primacy principle (what is learned first is learned best) or the structuring principle (later adult attitudes are structured by the orientations adopted as a child). However, these critiques are scarcely more sophisticated; they err in turn by reducing all possible theories of socialization to one necessary chain of connected assumptions. Marsh, for instance, lists three: "1) adult opinions are in large part the end product of youthful political socialization . . . 2) adult behavior is shaped in part by attitudes learnt during childhood socialization . . . 3) individual political opinions (and more specifically their political behavior) have an impact on the operation of a nation's government and political life" (1971, pp. 456, 460, 464). Similarly, Searing et al. (1973) contend that political socialization is influential only if children learn general attitudes that persist *unchanged* into adulthood and that structure specific issue beliefs acquired as an adult which in turn govern adult political behavior. Both articles contain two grievous misinterpretations: first, that political socialization theory requires invariant permanence of dispositions from childhood, and second, that socialization studies can, do, and must focus exclusively upon attitudes.[4]

The first misreading has a corollary misreading: Any change that can be located in adulthood can be taken as disconfirmation of the importance of childhood learning. Yet although it is true that some reworkings (Sears, 1975) contrasted a "lifelong openness" model with a "lifelong persistence" model, political scientists now recognize the necessity of reinforcement throughout the life cycle for a disposition to "persist" (e.g., Jennings & Niemi, 1981). Change may not then be a sign of the shallowness and lack of commitment with which individuals hold attitudes, but an indication of insufficient reinforcement. Furthermore, the implications of what is learned in early childhood may not be apparent until its application in adulthood, in which case fluidity and continuity coexist and may even be mutually reinforcing. An example is the impact of socialization upon the differential political participation of men and women; it is less a product of divergent learning about politics from a young age than the result of the first application of learned nonpolitical gender roles in adult politics (see Sapiro, 1983). Of course such "sleeper effects" are less obvious, but the impact of earlier learning is no less attenuated. Just as Easton and Dennis's (1969) critique of the functional approach to socialization points out that there is too much emphasis upon stability and replication between generations, so the same complaint may be made of those who expect absolute persistence with age. In either case, change is no automatic indication that the socialization process is malfunctioning.

Secondly, the revisionists' exclusive attention to attitudes as the sine qua non of the socialization process overlooks other dispositions toward politics which the child may form, and which may be more influential. Most important is cognition. This is arguably essential because, after all, individuals must know what something *is* before they can say how they feel about it, or whether or not they like it. Likewise, how one understands a phenomenon is directly linked to one's actions and reactions toward it. I do not here define cognition, as is often done, in the narrow sense of accumulated information about everyday politics like the number of senators, but more broadly, following the dictionary definition which underscores "the . . . process of knowing, including both awareness and judgment."

There are several reasons for focusing on cognition as a crucial and salient contribution of political socialization. First, such creation of political meaning would be an essential first step for the child seeking to comprehend and react to the political world. Indeed, even one of the first efforts in political socialization called attention to the most basic "orientations" which are acquired by children, including "the way the individual perceives and interprets political phenomena" (Hess & Easton, 1960, p. 633n.). Furthermore, the influence of early political learning seems to be more defensible here than is the case with relatively evanescent attitudes; the meanings of things are, after all, embedded in and reinforced by the verbal and symbolic languages we use which are contained in the culture around us—the socially perpetuated patterns of shared meaning which permit interpersonal communication. Finally, by setting it in the context of schema theory, the most recent work on political belief systems de-emphasizes the importance of affect-laden attitudes and has elevated the centrality of cognition in everyday political understanding and political action (see, e.g., Conover & Feldman, 1984; Sears & Citrin, 1982). The impact of cognition on political behavior is now clear; how and where people have developed the modes and content of political cognitions is far murkier.

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[4] Parenthetically, we may rest somewhat assured that Marsh's third assumption may be generally met. At least in the United States, recent studies show a close link between public opinion and political outcomes (Bennett, 1980; Page & Shapiro, 1983), although they also display some concern on where that public opinion comes from.

Although the study of cognition and cognitive development is then more justifiable than the study of attitudes among children, we still need to develop a strong theoretical rationale if we are to return political socialization to the deserved place of priority allotted it by Lindblom. Naturally, it is not very hard to claim that most of the political socialization research has been characterized by facile psychological theorization. But neither is it helpful to point this out without suggesting a more satisfactory approach.

Such an endeavor is particularly important, given the growing popularity of the model of cognitive development set forth by Jean Piaget.[5] In Merelman's apt phrase, "The developmental paradigm threatens at long last to become part of the conventional wisdom in political socialization and we all know how dangerous such a position can be to any research paradigm" (1982, p. 28). Piaget's theories of development were first introduced to political scientists in a theoretical essay by Merelman (1969) and have slowly gained increasing credence in the years since then. But Piaget as political scientists interpret his work is quite different from the original.[6] Indeed, a theme of this article is that Piagetian constructs of cognitive development are not very helpful in illuminating and explaining the process of political learning. First, Piaget's model does not fit even the few trustworthy empirical findings to date in political socialization. Second, there is a hidden bias in the ramifications of Piaget's model for political theory, as it more or less assumes individual learners to be responsible for their own learning and thus prevents us from scrutinizing a crucial problem of political socialization: whether it leads to social control or to self-determination. Lest history repeat itself with another set of misunderstood psychological theories, it is worth noting how Piaget has been construed—and misconstrued—in political science.

Piaget and Political Science

The most familiar invocation of Piaget is his well-known concept of developmental stages. In political science, this notion has been interpreted to signify strict limitations within particular age groups upon the sort of learning that can occur; in other words, children cannot be taught something before they are cognitively capable of learning it. This cognitive capacity is seen to be biologically determined. Thus, for example, Dawson, Prewitt, and Dawson (1977, p. 38) note, "From this [Piaget's] perspective, the child's political outlooks develop as his or her mental capacities develop. . . . The patterns of development are determined in large part by developmental processes innate to the child." The Piagetian model of cognitive development is then linked to "genetic maturation" alone and presented as the mutually exclusive competitor of social learning theory. Such a view is summarized by Merelman: "Men are social creatures; hence environmental forces constantly interfere with and contaminate genetic maturation. It is therefore impossible to determine the true extent to which such maturation regulates policy thought. Only if we could observe wholly uncontaminated subjects as they age would such an estimation become possible" (1971a, p. 1045).

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[5] The critique that follows is applicable to a renowned and oft-cited follower of Piaget, Lawrence Kohlberg (e.g., 1969, 1971), who has bypassed the relational emphasis of Piaget's (1932) own work on children's moral judgments to highlight the role of the growth of logic in moral development.

[6] It is admittedly difficult to point to one coherent body of thought that we can regard as Piaget's basic statement, although much of this synopsis is derived from his work on representational thought (Piaget, 1951). An astonishingly prolific researcher and author, Piaget's concerns shifted over more than 60 years of productivity from the relational development in *The Moral Judgment of the Child* (1932) to the more biological concentration of his late work (e.g., 1971, 1977). Useful introductions to his work include Piaget (1967), the anthology by Gruber and Voneche (1977) and, most delightfully, Bringuier's *Conversations with Jean Piaget* (1980).

A second use of Piaget is his designation of the child as an active participant in the learning process, which is borrowed to explain the apparent failure of parents and schools to socialize children into reproducing their exact attitudes. As, most notably, Jennings and Niemi contend, the child serves as "an independent and mediating influence in the socialization process" (1974, p. 331). In their work, they note that the political attitudes of their high school seniors may be determined at least partially on their own by themselves as much as by parents, peers, schools, and other agents.[7]

This introduction of the role of the individual learner into the process of political socialization according to particular stages of development is a welcome corrective to the earlier oversocialized notion of the child as a tabula rasa awaiting inscriptions from the environment. But this brief allusion to Piaget is incomplete, because it overlooks his inclusion of the social environment as a basic aspect of learning. Certainly Piaget can be faulted for underestimating the impact of the environment upon cognitive development, but his theories cannot be reduced either to biological determinism or total individual freedom within those biological limitations. Instead, a critical feature of his work is the necessity of organisms to adapt to their environments, both physical and social. It is intriguing to speculate what "wholly uncontaminated subjects" would look like if we could find any, but nothing develops independent of its surroundings. In Piaget's view, every interaction with the environment involves a dialectical relationship of accommodation and assimilation, which act, respectively, to modify and to preserve existing cognitive schemas. Individuals thus constantly attempt to adapt themselves to both external and internal reality. A social learning model is not opposed to a cognitive-developmental model but instead may be complementary or even subsumed by it.

A developmental model is then a useful perspective on the creation of ways to understand politics. First, such an approach, with its crucial role for how an individual assimilates outside reality, emphasizes the construction of a political belief system as a creative act, allowing for the considerable variability and idiosyncrasy between individuals which many observers have noted. Second, the inclusion of accommodation allows taking stock of the cultural factors which help to define the categories by which one orders the world. Finally, since the dialectical relationship between accommodation and assimilation continues through life in an equilibrium that is stable but mobile, there is a rationale for looking at the development across time of a political world view.

However, Piaget's developmental theory is inappropriate for studying the development of political understanding. Most notably, his principal concern was how children reach logical operations, pertaining to questions of space, time, number, physical causality, and similar concepts. If we conceive of formal operational thought in the political realm to indicate going from the possible abstraction to concrete reality, it is a truism that formal thinking does not characterize the political understanding of most citizens. Instead, the preferred way of political understanding appears to be intuitive, more akin to earlier concrete operations. Certainly cognitive development would lead toward the greater coherence, abstraction and hypothetico-deductive reasoning which are prerequisites for reasoned-out ideological thought. However, although progress toward formal ideologies in adolescence is well documented, it is by no means the dramatic breakthrough Piaget describes regarding formal operations, nor does it appear to be a universally attained stage (cf., Adelson, 1971; Adelson & O'Neil, 1966; Connell, 1971; Crain & Crain, 1974; Merelman, 1971a; Merelman & McCabe, 1974).

Piaget's theory is somewhat at a loss to explain the disuse of formal operational thought in the political realm. Although he never says so explicitly, his late works on equilibration (especially Piaget, 1977) sound as though the attainment of the final stage ought to occur in all domains. To Piaget, maintaining an equilibrium is the motivating force behind the behavior of all living organisms (Piaget, 1971). Because equilibrium is never definitively reached, one grows by searching for an ever greater equilibrium. To be sure, Piaget recognizes what he calls "horizontal decalage," the application of the same structures to different contexts and at different times—often noted by a lag of the development of moral or political reasoning behind other domains (Kohlberg, 1969; Merelman & McCabe, 1974). Nevertheless, Piaget suggests a push toward consistent levels of thought across domains, and more crucially, fails to consider why they may not be reached.

Some authors (most notably Connell, 1971, chap. 12) have attempted to salvage the Piagetian approach by noting the qualitatively different ways in which children interact with their physical environments as opposed to their social environments. Social surroundings cannot be manipulated like the plasticine used in Piaget's experiments, nor do they have a palpable physical presence that may serve to test or disconfirm the child's inadequate hypotheses. Instances where children can experiment with their social environment in order to understand it better are limited to the interaction between peers which produces collaborative constructions of interpersonal morality and friendship (see Piaget, 1932; Youniss, 1980). Understanding the workings of adult society cannot operate in a like manner, not only because of its untouchable distance and seeming permanence, but because the relations of adult to child do not focus on active exploration as a cooperative venture.

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[7] It should be noted, however, that Jennings and Niemi (1974) seem to use the cognitive developmental model only as an explanation of the residual variance that cannot be accounted for by social learning. In other words, cognitive theory explains only idiosyncracies that would otherwise be inexplicable to them.

However, the presence of disequilibrating experience is not the essential impetus to an individual's development in Piaget's theory. For one thing, as is frequently pointed out, many cognitive conflicts do not lead to cognitive growth. The true motivating force then must be the push supplied by the individual. Piaget visualizes the goals of behavior as not only mastering one's environment, but also extending one's understanding of the environment for precautionary means to avoid possible errors and disequilibrium in the future (see Bringuier, 1980). This substitution of actual experience by anticipated experience stresses assimilation over accommodation and shifts Piaget's emphasis from interactionism to innatism (see Broughton, 1981). As an ambitious attempt to extend Piagetian equilibration to social understanding claims, "Rather for progress to occur, Piaget postulates an *inherent* desire in the child to be part of the social world and to communicate effectively with other people" (Furth, 1980, p. 64; emphasis added).

If we are to follow Piaget, it is difficult to explain why individuals would then rely upon anything less than principled thought in figuring out politics. Piaget, after all, saw formal operations as the inevitable terminus of a process fueled by individuals' inherent curiosity about the world around them. It would follow that if this last level is not attained, then something must have blocked this process within the individual. At least this is what Merelman (1969) suggested: the less ideologically inclined must have suffered from punitive or distant relationships with their parents which forestalled identification with them and consequent cognitive development. But these speculations are not founded by empirical research (Sullivan, Marcus, & Minns, 1975). Piaget thus leaves us with an unhelpful conclusion —people do not understand politics in a principled way because they are less than curious about understanding politics—without being able to specify just what leads to this lack of interest in the first place.

I would argue that Piaget's focus upon the push supplied by the individual in moving development along underestimates the pull supplied by the social environment. The theory does encompass both accommodation and assimilation, but to Piaget, accommodation supplies only the content to be worked upon by the process of equilibration (see his comments in Gruber & Voneche, 1977, p. 836). It does not affect, at base, how that process operates, beyond slightly accelerating or, more often, decelerating it. Clearly, while cognitive development is a useful approach for political science, we require a model without Piaget's unjustifiable focus upon the primacy of formal operational thought. A hasty reliance of political scientists upon Piagetian theories would be no way to escape the bear market in political socialization.

An Alternative Model: Vygotsky

The theories of L. S. Vygotsky (1962, 1978) are a propos; they support the more laudable components of Piaget's pioneering work without accepting the emphasis either on the inevitable growth of logical operations or upon the individual as the prime motivating force in political learning. Vygotsky's work, which was left in something of a disarray when he died at the age of 37 in 1934, has been highly influential in Soviet psychology but has just begun to be of importance in the West after publication of recent editions of his essays. Although some have claimed that he is to conceptual development what Piaget is to logical development, their differences are acute. Whereas Piaget's interest is primarily biological, Vygotsky's is essentially sociological. Instead of underscoring the affinity of accommodation and assimilation across organisms as Piaget does, Vygotsky stresses the unique nature of the human mind, which is shaped by society, language, and social interaction.

To Vygotsky, language transforms the nature of development from biological to sociohistorical, rather than merely accompanying the biological process, as Piaget suggests. The social environment then provides a more central influence on the developing individual. How adults think is not merely the effect of their inventive capacities but also of their social experiences. The child's first use of practical intelligence is admittedly independent of speech, but at approximately age two, the *hitherto separate developments of thought and* speech converge to initiate an entirely new mode of behavior. Words, which are at first indistinct sounds, are discovered to be means to master their environment, as labels to permit children to single out a particular object, to focus their attention on it, and to mediate the immediacy of direct perception. In Vygotsky's words, children's first use of language allows the initial "perception of real objects": "By this term, I mean I do not see the world simply in color and shape but also as a world with sense and meaning" (1978, p. 33).

Only upon learning a language do children become capable of reconstructing perceptions in time and in space. As with Piaget's stage of symbolic play, children are now equipped with nascent independence from concrete visual presence. But only Vygotsky notes that language is a key tool of this liberation. Young children's speech begins as an accompaniment to action directed to the solution of the problem at hand. Even in earlier "protocommunication," as Vygotsky speculated and as recent research has borne out (see Damon, 1984, chap. 2), the meanings of an infant's motor movements are collaboratively created with adults who "recognize" a gesture and respond accordingly; the child may be the last one to realize consciously the meaning of her own gesture. But unlike Piaget's conclusion that action necessarily precedes thought, Vygotsky contends that actions become increasingly governed by the meanings imposed on those actions by language. Once children begin to master their surroundings with communication with others and the aid of speech, they can begin to master their own behavior in a like way. Even in the physical realm, manipulation of objects becomes spoken beforehand and eventually turned inward into an intrapersonal use of speech. Vygotsky argues that it is typical of every stage that operations at first accomplished by direct, interpersonal adaptation become carried out by indirect, internalized methods. Thus, he concludes, "the very mechanism underlying higher mental functions is a copy from social interaction; all higher mental functions are internalized social relationships" (in Kozulin, 1984, p. 108).

As purely "natural" perception is taken over by a more complex, socially mediated process, the mechanisms of speech acquire a new function in addition to labeling—that of synthesis. But young children's semantic memory (to use more recent terms) is far less developed than their episodic memory. Preschoolers can understand a problem and the goal set by it, but the conceptual strategies children use to solve them are radically different from those exploited by adults "in their composition, structure and mode of operation" (Vygotsky, 1962, p. 55). Their preconceptual attempts at grouping are determined largely by visual, concrete connections, beginning with the first phase of "heaps" where diverse objects are linked, virtually by chance, into a syncretic image. The next step to concept formation is complexive thinking, where, rather than abstract out one common attribute between objects, one relates only isolated attributes which may (and often do) differ as one goes from one object to another in the group. In all complexes, the concept cannot surpass its components but instead merges with the objects in an amalgam of general and specific.

The most common form of complexive thinking is what Vygotsky terms pseudoconcepts. The results of using a pseudoconceptual process may well be identical to those of using conceptual processes. But the actual procedures are quite divergent, just as Piaget's concrete operations can lead to the same endpoint as formal thought without traveling by the same avenue. Unlike genuine concepts, the pseudoconcept is complexive, based on horizontal sets of associations rather than a hierarchy of abstractions vertically running from general to specific. By contrast to Piaget, Vygotsky suggests that pseudoconcepts become the predominant forms of complexive thought through an interactive process of child and adult: "In real life, complexes corresponding to word meanings are not spontaneously developed by the child. The lines along which a complex develops are predetermined by the meaning a given word already has in the language of adults. . . . The language of the environment, with its stable, permanent meaning, points the way the child's generalizations will take. But . . . the adult cannot pass on to the child his mode of thinking" (1962, pp. 67-68). Communication between child and adult is assured because the words will usually refer to the particular objects each has in mind. But, to Vygotsky, because a word never refers to a single object alone but is a generalization defining a *group* of objects, it is in this grouping that the modes of thinking diverge between child and adult. "We might say that the child's and the adult's words coincide in their referents but not in their meanings" (1962, p. 73).

The ability to conceptualize involves more than mere unification and synthesis. It requires a selectivity and analytical ability in order to reach an abstraction that can become the main instrument of thought divorced from its elements; such abilities are not available before adolescence. But even then, previous modes of understanding do not become irrelevant. For one, every new stage is established on the elaboration of generalizations which dominated at the previous level. The new generalizations of previous generalizations may now reshape the meaning of the older concepts just as they were shaped by them before.

More important, while both Vygotsky and Piaget recognize abstract thinking as the final stage, only Vygotsky is precise in warning that this is but one mode of comprehension which may be used by the adult, and one, at that, whose use depends upon the social environment. Pseudoconceptual thought is not eradicated by the onset of true conceptual thought; it remains usable and useful in everyday experience. Compare Piaget, who suggests that earlier stages are completely replaced by those that follow, in which case early learning is much less consequential. Vygotsky instead implies that the level of thought that is tapped depends upon the nature of the problem to be solved—although there are certainly problems whose solution must await the acquisition of necessary skills. And given that abstract modes of understanding may actually be more cumbersome and time-consuming than the quick, straightforward solutions offered by concrete operations, pseudoconceptual thought can be an effective tool for making sense of the world at any age.[8]

Beyond that, Vygotsky claims that any level of development includes not only what individuals can do on their own, but what they can do with the help of more accomplished others. This capacity for handling more difficult tasks he terms the "proximal zone of development," which defines the abilities not yet fully matured but in the process of maturing. Piaget concludes, "Everything one teaches a child one prevents him from inventing or discovering" (in Bringuier, 1980, p. 102). But Vygotsky sees teaching as essential in setting forth challenges to the limits of one's capabilities which pull development along as much as individuals push it along by themselves.

In short, to Vygotsky, cognitive development is not, by and large, an internally motivated and internally regulated process. Such a model is increasingly accepted within psychology, particularly given the recent wave of research crediting preschoolers and even infants with skills and capabilities previously thought beyond their grasp. Unlike Piaget, who assumes that certain rules and methods are not available to the child unless spontaneously utilized, we are beginning to see that such rules and methods may indeed be available given encouragement, training, or simplification of the task. As Gelman (1978) points out in her review of the pathbreaking work that questions the Piagetian paradigm on preschoolers' skills, we must make a distinction between the spontaneous deployment of rules and the *capacity* to deploy them. Whether or not one exploits a cognitive ability in a particular task may be at least as much a function of the task as of the individual.

The implications of Vygotsky's model for the development of political thought are considerable. Rather than blame the individual for failing to develop into a rational political thinker, one might implicate the social environment for not providing an adequate pull. In a phrase, if the social environment does not challenge current forms of political thinking, it reinforces the use of the less complex modes of understanding. As long as politics is of peripheral importance, a vague fit produced by an intuitive (or pseudoconceptual) thought process is more than adequate. Only when politics becomes more central in one's existence would a pull provided by one's environment impel one to a higher level of reasoning and understanding.

Vygotsky's Cognitive Developmental Model Applied

A model derived from Vygotsky is highly useful. Unlike Piaget he can explain why most people do not go beyond intuitive comprehension of politics, while agreeing that understanding evolves through a dialectic process of balancing off accommodation to external reality and assimilation to internal reality. In addition, in pointing to the crucial role of the social environment in cognitive development, Vygotsky suggests that the political learning process has no biological endpoints and indeed may never end. Finally, what reliable evidence we have on political socialization —that from semistructured interviews, hypothetical dilemmas, and semiprojective tests with young subjects—is consistent with this framework. This is not to say that Vygotsky's cognitive-developmental model can be more than a hypothesis to be tested, particularly with the numerous lacunae in political socialization. However, it fits Langer's criterion for bypassing other approaches: "Limitation is not itself a reason for rejecting a theory. . . . One [must have] a more powerful notion, which will take the constructive work of the previous one in its stride, and then do something more" (1953, p. 57).

To begin at the beginning, although we know little of the precise interconnections made by children between political and nonpolitical authority, a preschool child's understanding of political phenomena is vague and not separable from other realms of life, actual or imagined. Connell's discussions with five- and six-year-olds convincingly suggest that "political figures, cartoon figures, fact and legend jostle each other with splendid promiscuity" (1971, p. 11). Yet children at this age are not lacking in understanding of many aspects of social life which may serve as bases for later comprehension of the outside adult political world.[9] As early as preschool and generally by second grade—the traditional early demarcation for political socialization—children have remarkably full and perceptive understanding of authority, rules, and cooperation in their immediate worlds, including considerable awareness of the conditional and contextual nature of authority (see, e.g., Damon, 1977; Turiel, 1983; Youniss, 1980). The contrast between the well-developed understanding of nonpolitical adult authority and the jumbled sense of government and politics illustrates clearly the centrality of proximate social interaction in boosting development to higher levels of comprehension.

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[8] This has become increasingly clear with the numerous illustrations of "U-shaped behavioral growth," where a skill at a particular task may be at least momentarily lost upon moving to the next stage, when new methods are not yet efficiently used. The classic statement here is Karmiloff-Smith and Inhelder (1975). For indications of adults' preferences for concrete rules of thumb in social cognition, see Nisbett and Ross (1980).

Around the time children begin first grade, they are able consistently to visualize politics and government as separate and unique for the first time. This first transition to pseudoconceptual comprehension of outside politics begins with a preliminary grasp of the concept of "government," which one team of researchers claims from semistructured interviews, "constitutes a recognition threshold which provides the child with a handle by which he or she can begin to respond to a wide variety of questions requiring politically germane information" (Moore et al., 1976, p. 423). In Vygotsky's terms, children at this age have discovered a word by which they can focus their attention on political phenomena, and separate out more coherently the "heaps" of images they have been amassing.

However, because the child prefers concrete understanding, the portrayal of government is almost invariably personalized. And such an understanding of the concept of government is not conceptual because it is organized around a synthesis, not an analysis, of objects. Just as other social roles are defined for children by functions the individual performs (Furth, 1980), the child first comprehends political actors by creating, in Connell's terms, a "task pool" of any and all actions known to be done by officials, who are distinguished by being both "special" and powerful (1971, ch. 2). At first, all politicians are seen to perform the same tasks; only with age is this indistinct task pool differentiated among them (Greenstein, 1975). This pseudoconceptual image of government cannot help children to differentiate between political objects but does allow some initial success at separating governmental from nongovernmental phenomena.

Young children's comprehension of politics thus begins with tangible prototypes of governmental objects seen to interrelate with personally familiar life spaces. This image is not only personalized but is more idealized than those reported by older compatriots. It is true that the glowing idealization of the early studies was severely exaggerated by their reliance on survey methods, but indications of greater positivity toward political authority among younger subjects have been noted by both semistructured interviews (Connell, 1971; Furth, 1980) and hypothetical dilemmas (Adelson, 1971; Crain & Crain, 1974). Political authorities are not children's superheroes, which the first examinations implied, yet their power, in Connell's terms, is not considered "problematic."

As Connell (1971, p. 28) contends, such positivity may be based in cognition rather than emotion, particularly because there are striking similarities between how children view ideal political and nonpolitical authorities. Recent studies in social psychology suggest that children perceive the adult world quite differently from the one they can create with their peers. For instance, being kind to another child is thought of as sharing; being kind to an adult is seen as obeying (Youniss, 1980). This set of expectations may be taken as given by young children in their dealings with any and all adults, as part of a reciprocal arrangement whereby adults will be kind (i.e., providing and helpful) if children will be kind (i.e., obedient). The extension of the child's general view of adults to political authority is evident; the early conception of political leaders is then a subset of this overarching understanding of child-adult relations, which may have considerable intercultural commonality because the resources each brings to interactions are inevitably unequal.

However, if there is a universal developmental tendency for younger children to fail to criticize political figures, the rapidity of its decline depends on the culturally based portrayal of political authority. In the United States, the tendency would be reinforced by the cultural "positivity bias" (Lau, Sears, & Centers, 1978) which helps to perpetuate such favorable perceptions, whereas in France the cultural suspicion of politics and the clear opposition of familial and political spheres (Wylie, 1957) would make any idealization lower compared to other cultures and shortlived with age (see Greenstein, 1975). In short, the development of political understanding clearly appears to be a product not merely of simply the child's level of mental development or simply the messages provided in the sociocultural environment, but the *interaction* of the two.

Concrete modes of political thought dominate at least through early adolescence, even while the image is elaborated and extended beyond the immediate environment. The application of abstract principles sometimes may appear as early as the first years of adolescence, but the tendency is usually toward tangible and often personal implications of political objects. For example, costs and benefits of proposed governmental programs are seen less in terms of consequences to society than in their effects on lone individuals (Adelson & O'Neil, 1966; Merelman & McCabe, 1974). The concepts of the society and of the polity are still no more than the sums of their increasingly disparate parts. Synthesis has not yet been succeeded by analysis.

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[9] Recall Knutson's (1973) discussion of "pre-political ideologies," which is underscored by Conover and Feldman's (1984) demonstration of close links between one's general philosophy of human nature and one's specific political stances and beliefs.

Adolescence provides the first opportunity to deal with politics on the level of formal operations. In their ability to visualize other worlds beyond the present one, and to comprehend concepts such as government in an abstract manner, adolescents clearly *can* think about political objects in a qualitatively different mode than their younger counterparts. Yet older is not necessarily better. As the most balanced insight into the process of political development at work among adolescents suggests (Merelman & McCabe, 1974), the use of formal rules is not only associated with reasoning capacity and age, but also the individual's level of politicization and the familiarity of the political problem at hand. Principled reasoning is apparently most stimulated by the interaction of the level of overall cognitive development with high political interest on an issue area which has no intuitive responses to draw on —a rare combination! As Vygotsky's model suggests in contrast to Piaget's, adolescents may have a latent capacity to analyze politics in a principled manner, but its use depends on whether the environment provides a pull within this proximal zone of development, especially by its politicization. Merelman and McCabe's conclusions reinforce my earlier points: imaged modes of comprehension are still very useful when that is all that the environment requires, and the push supplied by the individual is an insufficient explanation for the advancement of political thought processes.

The process of political development then does not stop with adolescence. Indeed, a key virtue of Vygotsky's model is that learning does not end at adulthood; continued fluidity of political points of view is acknowledged alongside the centrality of early learning in childhood, because of Vygotsky's essential role for the social environment. How adults think of and conceive politics is likewise a function of the extent to which messages from the environment reinforce or challenge their prior expectations (see Jennings & Niemi, 1981). Change is less common than continuity in adults' political conceptions, not because they are affectively committed to them but because they are seldom challenged. Of course, drastic changes in one's social and political environment or new experiences during adulthood may well provoke a shift in one's political perspectives but cannot be guaranteed to do so. When political perspectives are challenged, an adult can respond by restructuring that environment, such as selecting friends and even spouses who are politically compatible and who will help to maintain potentially threatened views (Finifter, 1974; Newcomb et al., 1967).[10] But in modern societies which deemphasize full political involvement, confirmation and reinforcement of one's loosely structured ways of making sense of politics may be far more forthcoming than are challenges which would necessitate fuller, more conceptual modes of thought (see Bennett, 1980; Edelman, 1964). In adulthood as in childhood, the meanings taken from a given experience derive from the interaction of assimilation and the individual's schema with accommodation to the changing political reality.

In sum, Vygotsky's cognitive-developmental model provides a sound perspective on political learning which provides one part of the solution to the bear market. Of course it does not set forth an all-encompassing psychological theory which elucidates every aspect of political socialization. As Rosenau's (1975) earlier essay cautioned, cognitive development can tell us only about how means and modes of making sense of the world are created and developed. It is indirectly linked to emotional development or to the learning of particular behaviors, either of which would be more precisely addressed by other psychological models. One must admit the limits of cognitive development; it is no panacea for all the ills of political socialization. At the same time, a good case can be made for cognitive developmental theory as being, if not the most basic, the most significant psychological model of learning, for the simple reason argued above that political cognition may be crucial to political behavior.

Conclusion: Where Next?

Most important, Vygotsky's model is not only valuable for assessing where this research has been but for pointing out where next to go. In particular, with its guidance, several vital questions may be posed which are consequential not just for psychological theory but for political theory. After all, our choices of psychological theories of learning should be guided by our choices of political theories, and vice versa. No psychological model of development is politically value free.[11]

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[10] We might add that such selection of friends on the basis of political compatibility is not exhibited among high school students, who rarely discuss politics with each other (Jennings & Niemi, 1974; Sigel & Hoskin, 1981).

Recall that the initial view of the political learning process was capsulized by Hyman's (1959) term, "political socialization." As Merelman (1972, p. 135) notes, Hyman's synthesis was influential in imposing his assumptions from mainstream sociology that "human development is almost exclusively a process by which the environment, conceptualized in terms of social agencies, impresses itself upon the helpless, formless child." This focus on what the child is taught matched the contemporary political theories that argued that for a political system to survive, citizens needed to be trained in particular attitudes and behaviors which would be functional. The research questions consequently asked what the principal "agents of socialization" were, and looked less at how children and adults change and grow politically than at whether (or not) they internalized the particular values, norms, and attitudes that were deemed necessary for democratic citizenship.

As Sears (1975) points out, this preoccupation with how a political system maintains itself neglects the impact of political learning on individuals' effectiveness in pursuing their own interests. Instead of being primarily concerned with what is best for the system, we may wish to note what would be best for the full development of the individuals within that system. Such a theoretical focus leads easily to cognitive-developmental models of learning.

Both Sears (1975) and Lindblom (1982) suggest that we must assess not only whether socialization produces benign systemic effects but more restrictive personal effects as well. To state Lindblom's argument more explicitly, we ought to turn to a central and severely underexamined question of political socialization: is it a laissez-faire process of learning, whereby one chooses to know, feel, and think about politics as one wishes; or is it a process of inculcation and enculturation controlled more by the teachers than the learners? Despite its merits, the Piagetian model does not allow us to investigate this question. With its emphasis upon the individual as the primary impetus to development, the natural conclusion would be that if one does not develop to more conceptual ways of making sense of politics, it is one's own fault. But as long as the sociopolitical environment is a critical influence on not only what but how we think about politics, Vygotsky's model is more suited for our purposes, because it squarely addresses the interaction of individual and society and convincingly notes that the nature of the challenge helps to determine the nature of the response at all ages and all stages.

Finally, Vygotsky's model provides a research agenda for political socialization which is distinct from the questions more commonly asked. Indeed, by emphasizing cognitive development within a sociopolitical environment, it points out two questions *not* to ask—the familiar problems of who or what is *the* dominant "agent of socialization" and what is *the* crucial period for political learning. A cognitive-developmental model treats these matters as less important, because learning is viewed as a slow process of accretion and elaboration as individuals gradually grow within their environments. Every interaction with the environment tests and refines one's expectations in the ongoing dialectic of accommodation and assimilation. Certainly the shape of the expectations and the types of tests that are applied depend upon one's stage of development. But we need not spend time arguing about which stage is most crucial; as long as we do not assume that earlier stages disappear upon attaining a more conceptual level, they are still available for use during adulthood, if that is all that the environment requires. Learning during childhood may be crucial for basic pseudoconceptual conclusions about the political world in general, which will continue into adulthood as bases for understanding if reinforcement is forthcoming. But learning during adulthood may be crucial for refining these general reactions so as to guide political behavior more effectively and directly. In short, both processes are essential; we should be examining them both.

Contrasting the influences of different agents of socialization also becomes less pressing when we include the learners as active participants in their own development. Such an argument is, of course, underscored by the apparent failure of single agents to achieve much success in transmitting particular political orientations. But alongside these empirical considerations, the model's emphasis upon individual initiative as it interacts with and is encouraged or discouraged by the outside environment implies that no single agent will be dominant across all individuals, as different persons respond to different environments in different ways. Naturally, if sources of political learning are mutually reinforcing over time, there is less possibility that the individual will question the messages received and even less reason to search for "the" principal agent of socialization.

If we are to bypass the traditional questions of political socialization, Vygotsky's model focuses our attention on several concerns which have been overlooked. There are methodological guidelines to be followed, too. First, in studying children, we should abjure the stock in trade of the political socialization scholar, survey research, particularly pencil-and-paper questionnaires. The quality of these easily collected data from large samples must be viewed as, at the very least, doubtful. The alternatives, like semiprojective tests, unstructured or semistructured interviews, experiments and Q-sorts, are time-consuming enough that we may have to settle for smaller, less nationally representative samples. But, as Brown (1980) has noted, generalizing results from small samples is less problematic as long as sample populations are chosen for reasons of interest to theory. Second, few studies of cognitive development of political understanding (but cf. Adelson, 1971) have followed the same subjects over time, but instead infer development from cross-sectional samples at one point. Clearly, we need panel studies of individuals, and from different ages, in order to extricate life-cycle and period effects in changing political cognitions. Third, defensible studies are lacking on subjects other than white, middle-class children, often from private schools. We might have once considered ourselves aware of *what* individuals think in a plethora of social, ethnic, racial, regional and class contexts in the United States; but *how* they think about politics is an absolutely open question. Fourth, in examining the impact of the social environment, we have depended far too much on either self-reports of styles of interaction (e.g., Chaffee, McLeod, & Wackman, 1973) or on agents' political attitudes alone (e.g., Jennings & Niemi, 1974). Direct observation of parents, teachers, and peers is absent from political socialization but is essential if we are to gauge the complex influence of social interaction upon political learning.

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[11] In the apt words of Lawrence Kohlberg, "A psychological theory of ethics (or of cognition) is incomplete, even as a psychological theory, if its philosophic implications are not spelled out" (1971, p. 154).

Although it is too global to provide specific hypotheses, applying Vygotsky's model in political science emphasizes three larger questions which illuminate Lindblom's hypothesized contrast of self-determination versus social control and which point the way for future empirical research. *First, we should investigate whether sociopolitical environments of individuals help to challenge or to maintain the vague intuitive notions about politics which are first constructed in childhood. and whether the different messages are reinforcing or conflicting.* Vygotsky's emphasis upon the pull provided by the overall social environment on cognitive development necessitates going beyond the usual sources (parents, peers, and schools) to examine the political communities and cultures in which individual agents are based and in which people come of political age. Connell's (1972, p. 330) trenchant critique of the literature on the family as an agent of socialization contends that we should view their contribution as providing a social nexus shaped by extrafamilial forces. "It appears that older and younger generations have developed their opinions in parallel rather than in series, by similar experiences in a common way of life." Yet there have been remarkably few attempts to depict this common way of life and, indeed, to ascertain how common it really is. Sources of conflict and concordance in the entirety of possible socializing agents and experiences should be carefully noted, as well as the extent to which later learning provides much, if any, challenge to earlier means of understanding politics. Vygotsky would predict that an environment which by its very heterogeneity provides challenges provides an impetus to the development of political understanding. Vygotsky would also contend that social interaction per se may be a more important influence on developing political understanding, as some studies (e.g., Chaffee, McLeod, & Wackman, 1973) have already suggested; but an alternative hypothesis would suggest that regardless of how social interaction is carried out, the politicization of the environment is more crucial. Finally, individuals also contribute to their own political socialization; selective interpretation and selective perception can occur even in strongly consensual environments. To follow Newcomb et al. (1967) and Finifter (1974), we need to examine whether even young individuals help shape their environments to ensure that they learn only what they want to learn, and the circumstances under which such control is exerted. In sum, future studies should give equal time to both accommodation and assimilation.

Second, we should study how the messages derived from sources of political learning are determined. In the panoply of studies of parental socialization, it is astonishing that no American study to my knowledge has done what Percheron et al. (1978, chap. 1) did in France—ask parents their own perceptions of how political education is and ought to be furnished. Even with more formal sources of political learning such as schools and the news media, political scientists tend to treat the ultimate messages as faits accomplis, although there are competing explanations—political, economic and cultural—which have varying theoretical implications. With the considerable attention to cultural variations as an explanation of differential patterns of socialization, the process of *how* a culture goes about influencing individuals is left extremely indefinite. Do individuals who end up socializing others into politics partake of conscious philosophies of political education? Do they have even vague standards of what are appropriate and inappropriate ideas to which children should be exposed? In other words, does any greater influence on political development occur when, as Vygotsky's model would highlight, the environment is consciously political as opposed to when it is nonconsciously apolitical? With few exceptions (e.g., Litt, 1962; Merelman, 1971b), there are no examinations of how closely sources of political learning mirror the perspectives of the community around them, and the extent to which, in Connell and Goot's (1972/1973) phrase, politics must be invoked not merely as the outcome of political socialization but a cause thereof as well.[12]

Third, we should pay closer attention to the links between competence and performance in political behavior at all ages and all levels of development. In psychology as well as in political science, competence and performance have too frequently been conflated (see Gelman, 1978). However, Vygotsky's model, especially by its excellent thesis of the proximal zone of development, accords the distinction far more importance than does Piaget. Indeed, if we are to assess not only the practice but the possibilities of politics, investigations must study not only what skills persons lack but also those that they possess. This oversight is particularly evident in studies that discuss the child's "progress" from naivete to realism (e.g., Easton & Dennis, 1969), with much less attention allotted to what may be lost in the socialization process, such as political imagination (Connell, 1971) or life-supportive concerns (Crain & Crain, 1974). In highlighting capacities, however, we should be careful to avoid the pitfalls of previous research, which, probing beneath the surface into the *process* of thinking about politics, have used a variety of hypothetical situations and dilemmas which cannot convey how respondents would perceive and deal with everyday political phenomena. As studies in moral development witness, it would undoubtedly be naive to contend that hypothetical capabilities will be automatically translated into actual behavior, given the complexity of contextual factors. Yet it is also misleading to assume that persons can perform no better than the abilities they in fact demonstrate in everyday life. Indeed, Vygotsky's model would predict more sophisticated responses when the individual is assisted in the task, either by group processes or by crafting the information to provide the greatest challenge to pre-existing schemas. But this contrasts to another possibility that persons do not show any greater capacity in hypothetical situations than in real-world contexts —indeed, as Haan's (1975) study suggests, the reverse may be more accurate if the actual context somehow cathects one's political perspectives. In sum, research must examine both how individuals conceptualize political phenomena *and* also obtain a precise comparison to their views and behaviors toward everyday contemporary politics.

In short, we may have to be ambitious if the development of political understanding is considered to be a central concern of political socialization and, if we follow Lindblom, of political science. Evaluating the political socialization process requires focusing at once on the messages provided by sources of political learning, the determination of those messages, how they are understood and processed by the learners, and the potential capabilities of those learners—all of this studied as subjects age in different social and political contexts. Such a research agenda may well seem daunting. Yet a scientific enterprise requires its practitioners to do more than establish where we have been after the fact, the case with political socialization where the split between empirical research and theoretical assessment has been especially severe. We should also have a sense of where we are going and why, not only in terms of what political theory suggests is consequential, but also in a way that reflects being informed by psychological models of learning and development. After all, if it is true that the elaborate structure of political socialization came close to collapsing in recent years because of the infirmity of its foundation, we must then begin with some basics if we are to fix it up once more.

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[12] We might even go one step beyond that, in arguing that the political system or, as Greenberg (1981) has convincingly suggested, the market economy might also be direct agents of political socialization. Special attention ought to be paid to the effects, which recent work on power has suggested, of the economic and political structure upon socialization into powerlessness—what one might felicitously term "apolitical socialization."

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Control and Feedback in Economic Regulation: The Case of the NLRB

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This article presents an empirical analysis of the National Labor Relations Board, focusing on the balance the agency strikes between the interests of business and labor. It is oriented by a theoretical framework that, relative to popular models, takes a broader view of the causal structure of regulatory performance—one that simultaneously allows for presidents, congressional committees, the courts, agency staff, constituents, and economic conditions. The empirical results are instructive. All of these factors prove to have significant impacts on NLRB decisions. In addition, the core regulatory actors —Board members, staff, and constituents—are shown to engage in mutually adaptive adjustment: each is responsive to the decisions of each of the others, and their reciprocal relationships impart equilibrating properties to the system as a whole. Thus, the evidence points to a varied set of important determinants and to the dynamic nature of their interconnection. To the extent that these findings are at all characteristic of other regulatory agencies, simple popular models of regulation are likely to give anemic explanations, if not highly distorted accounts, of why agencies behave as they do.

Government regulation has long been viewed as a controversial and important matter, yet it has not been well studied, at least not in a balanced way. The social science of regulation has done an enthusiastic job of investigating the kinds of technical issues in which economists are most interested: rate setting, cost determination, efficiency, optimality. Far less effort, particularly in the form of quantitative analysis, has been devoted to even the most basic issues of politics and organization. What can we expect of the relationship between regulatory agencies and the groups they are supposed to be regulating? To what extent and toward what ends are these agencies controlled by elected politicians? What are the determinants of agency autonomy, and how is it exercised in the making of economic policy? Central as these questions are to our understanding of regulation, we are in no position to offer satisfactory answers. At this point, the necessary empirical foundation simply is not there.

Despite the lack of evidence, or perhaps because of it, theories of regulatory politics abound. The most prominent is the capture theory (Bernstein, 1955; Huntington, 1952; Peltzman, 1976; Stigler, 1971), which, in asserting the responsiveness of bureaucratic agencies to clientele groups, has several variants (see Mitnick, 1980) as well as implicit links to more broadly based theories of administrative behavior—for example, interest group liberalism (Lowi, 1969), cooptation (Selznick, 1949), and corporatism (Schmitter, 1974). But no version of the capture theory is universally accepted, and it is increasingly under attack by those who dispute both the pervasiveness of the capture phenomenon and its proffered explanations. The alternatives are many and varied, with emphases on the increasing pluralism and turbulence of regulatory environments (Sabatier, 1975; Weaver, 1978), the bases of bureaucratic autonomy and discretion (Katzmann, 1980; Niskanen, 1971; Wilson, 1980), and the mechanisms of control available to political institutions—most often to legislative committees (Barke & Riker, 1982; Calvert & Weingast, 1984; McCubbins, 1984; McCubbins & Schwartz, 1984; Weingast, 1984; Weingast & Moran, 1983), sometimes to the executive (Beck, 1982; Chubb, 1983; Moe, 1982) or the courts (Melnick, 1983).[1]

In addition, a related but distinct body of work has emerged in the last several years, much of it addressing questions of political control, comparative in substantive focus, and founded (often implicitly) on optimal control theory and its associated reaction-function methodology (see Alt &

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[1] See also Kiewiet and McCubbins (1985, in press) on budgeting and Chubb (1984) on federalism for evidence on political control of the bureaucracy in these areas.

Chrystal, 1983; Alt & Woolley, 1982). Beyond its more abstract contributions, this work has generated interesting notions of relevance to regulatory behavior—for example, that political parties and incumbent politicians often succeed in controlling economic outcomes to please coalitional supporters (Hibbs, 1977) and tend to produce "political business cycles" in various areas of governmental activity (Nordhaus, 1975; Tufte, 1978). But these notions, too, are matters of dispute.

The political theory of regulation is in an early stage of growth and development. Capture, legislative control, agency autonomy, political business cycles—all are surrounded by controversy and uncertainty, and underpinned by competing methods, approaches, and assumptions. To a large extent the current disarray is what we should expect in view of the vigorous generation of ideas over recent years, and it is hardly lamentable, for it clearly provides exciting opportunities for advancing our knowledge of government regulation. Yet it seems fair to say that important forces for integration and coherence have thus far played a smaller role here than they should have, primarily because so much of the influential work (particularly work by economists) has tended not to build in a systematic way on what we already know about public bureaucracy.

Specifically, it is plain from decades of research on bureaucratic politics that public agencies are anchored in networks of relationships with executives, legislative committees, and constituency groups, and that their behavior is shaped as well by their economic environments, the courts, and a range of internal organizational factors. Although this is no secret, popular models of regulation as well as quantitative empirical work have tended to focus only on very small parts of the whole—in the former case for reasons for clarity and mathematical tractibility, and in the latter because of data collection and measurement problems (and because they are often guided by these same models). Given the very real difficulties that comprehensiveness entails, these approaches are entirely reasonable short-term research strategies. But it is important to remember that they threaten to yield biased inferences about the causes of regulatory behavior. They clearly omit factors whose causal effects may overwhelm or distort the "special" relationships on which they singularly focus. Moreover, it is common in a data-poor world for myopic models to take on lives of their own, and thus for capture or congressional dominance or budget-maximization to attract ardent followings for reasons that have little to do with demonstrated empirical validity. Ultimately, there is no substitute for pursuing more comprehensive, better specified theoretical models and empirical tests (Joskow & Noll, 1981).

In this article, I take a step in that direction through an empirical analysis of the National Labor Relations Board, with a substantive focus on the balance the agency strikes between the interests of business and labor in its unfair labor practice decisions. The task is to explain why its decisional balance fluctuates as it does over time. This is approached by means of a theoretical framework that, relative to popular models, takes a broader view of the causal structure of regulatory performance: by incorporating an array of political, economic, and organizational forces on agency behavior, and by recognizing that these forces constitute a hierarchically ordered system of relationships. In this way, it is possible to investigate simultaneously the influence of presidents and congressional committees, the constraining role of the courts, the impact of economic conditions, the internal decisions of agency staff, and the grievance-filing behavior of business and labor—and to do so by taking account of their locations and interdependence within the regulatory system they constitute.

The empirical results are instructive: All of these factors prove to have significant impacts on NLRB performance. In addition, the central regulatory actors—Board members, organizational staff, and constituents—are shown to engage in mutually adaptive adjustment: each is responsive to the decisions of each of the others, and their reciprocal relationships tend to impart equilibrating properties to the system as a whole. Thus, the evidence points to a varied set of important determinants and to the dynamic nature of their interconnection. To the extent that these findings are at all characteristic of other regulatory agencies, myopic models are likely to give blurred explanations, if not highly distorted accounts, of why agencies behave as they do.

My general purpose in all this is to illustrate the value of (relative) comprehensiveness for exploring basic issues in the politics of regulation. But I also have a more specific theoretical axe to grind, one I want to be clear about from the beginning: I think presidents often play major roles in shaping regulatory behavior and that they are systematically ignored by popular models, the capture and congressional dominance models in particular. Although presidents are but one part of the overall causal structure to be investigated here, I will make a special effort along the way to highlight the evidence on presidential impact in order to help remedy what I view as an unwarranted bias in these familiar theories.

Some Background on the NLRB

Public bureaus tend to be shaped by general types of forces. Politicians mandate goals, structure, and resource levels, and attempt to control agency behavior. Constituency groups generate support, opposition, and workloads. Task-performance is adapted to social and economic conditions. Upper-level decision makers must rely upon lower-level employees, who control crucial information and make important choices for the organization. The list of commonalities could easily go on. Yet the specifics will clearly vary from bureau to bureau as the inevitable variations in history, task, structure, and other factors lend partial distinctiveness to each bureau and its situation.

The NLRB was a quintessential product of the New Deal.[2] It was created by a Democratic president and Congress as an administrative means of stabilizing labor-management relations, and, as such, was part of a much larger attempt to regulate and manage an economy that had gone spiralling out of control. It was also the centerpiece of an explicit effort to enhance the power of labor in the political economy by nurturing the growth of unions. The vehicle was the National Labor Relations Act of 1935, also known as the Wagner Act. This bill set up a three-member Board with responsibilities in two general areas. The first involved supervision and regulation of union "representation elections," the new procedural means by which workers decided whether they wanted to be represented exclusively by a particular union. The NLRB's job was to ensure that statutory and administrative rules were followed in representation elections and the campaigns preceding them, to adjudicate disputes about whether these rules had in fact been violated, and to pass judgment on the outcome. The second dimension had to do with unfair labor practices: acts committed by an employer—for example, the dismissal of employees in retaliation for their union activities—that were now declared illegal by Section 8A of the Act. Here, the NLRB's task was to adjudicate charges brought by unions and workers against employers and to require remedial actions.

In its first decade or so of life, the NLRB was a zealously prolabor agency, staffed at all levels by people philosophically in support of unionism. But all of this changed when the Republicans took control of Congress in 1947. One of their top priorities was to modify the Wagner Act, clean house at the NLRB, and reorient its behavior toward an accommodation of the "legitimate" concerns and interests of business. In effect, these goals were accomplished with the passage of the Taft-Hartley Act that same year. Among other things, this legislation abolished the closed shop, allowed states to pass right-to-work laws, expanded the Board to five members, and made the agency's General Counsel independent of the Board and a presidential appointee. Most important, given the data base of this analysis, Section 8B defined certain acts by unions as unfair labor practices—for example, secondary boycotts, pressuring employers to fire unfavored workers. For the first time, then, the NLRB was in the position of having to prosecute and adjudicate charges brought against unions by employers and workers. These changes so conflicted with pervasively held staff views about the NLRB's prounion mission that many soon left the agency. The crusade was over.

Subsequent to Taft-Hartley, the NLRB adapted to its new mandate, to the altered expectations of politicians and constituents, and to the more professional and less philosophical outlook of its own personnel by striking a more moderate balance between the interests of labor and business. Since then, no one has seriously claimed that the agency is captured by unions. The political controversy and passionate disputes that surrounded the NLRB in its early years have subsided dramatically, and for the last two decades it has rarely been a salient focus of partisan politics. Indeed, it has come to be regarded by observers on both sides of the political fence as one of the most professional, efficient, and successful of government agencies, processing with fairness and dispatch untold thousands of cases every year.

Still, the central fact of life even for a "professional" NLRB is that it is continually forced to choose between labor and business. And it is in the processing of cases, of course, that the labor-business balance is ultimately determined. What is the nature of this process for unfair labor practice cases? It begins with a charge, filed with a regional office of the NLRB, that a union or employer has committed an unfair labor practice. This charge is then investigated by staff members of the regional office, who recommend a formal complaint upon evidence that the charge is meritorious; where such evidence is lacking, they recommend dismissal. This recommendation is accepted (almost always) or rejected by the regional director, whose decision can in turn be appealed (but rarely with success) to the general counsel in Washington. The general counsel's decision is final and cannot be appealed to the courts. Few cases go through all these steps. The vast majority are dismissed, voluntarily withdrawn (usually in anticipation of formal dismissal), or—with active encouragement from the NLRB, eager to manage its horrendous caseload

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[2] This section offers a quick overview of NLRB history and procedures. For a thorough discussion, see McCulloch and Bornstein (1974) and Gross (1981).

—handled through voluntary agreement between the disputants.

The rest, those leading to formal complaints, go into adjudication. With NLRB lawyers representing aggrieved parties (i.e., prosecuting) and defendants hiring their own counsel, cases are heard in trial-like settings before administrative law judges. The losers can (and usually do) appeal to the Board, which, in order to hear so many cases, is broken down into five three-member panels (with each Board member sitting on three panels). Most cases are heard and decided by one of these panels; a much smaller portion is considered by the full Board, often cases considered important or controversial. All decisions can subsequently be appealed to the courts for review, and a large percentage in fact are. Should compliance with the Board's final orders prove to be a problem, the NLRB must go to court for enforcement.

In sum, the NLRB regulates labor-management relations largely by processing constituent-filed grievances according to set procedures, and it makes decisions subject to constraints by politicians and the courts. Although the details of history, mandate, and organization give the agency its own distinctive qualities, it is in most fundamental respects rather typical. If it departs markedly from the norm, it is in the nature of its constituency, which is emphatically bipolar and highly organized with strong links to parties and politicians. However, once we move from its turbulent early period of zealously prolabor regulation and partisan conflict into the post-Taft-Hartley period of professionalism and moderation, the NLRB loses virtually all of its overt political salience and essentially joins the rank-and-file of federal agencies. Given its constituency characteristics, the modern NLRB may in some respects be more politicized and more closely scrutinized than most agencies, but there is no reason to think the differences are dramatic.

Theoretical Framework

My explanation of NLRB performance is oriented by a framework in which all participants are assumed to be rational and self-interested (with self-interest broadly construed). I prefer to call it a framework rather than a model because I will be making only the most general assumptions about how these actors are motivated and interact with one another. My purpose is to impose a coherent structure on the regulatory context, one that highlights basic relationships and the underlying flow of causality without adopting positions on specific substantive issues that, at this point, are matters of legitimate debate.

This framework is a simple reflection of two general lines of thought about bureaucratic behavior, the Simon-March behavioral tradition of organizational analysis (Cohen, March, & Olsen, 1972; Cyert & March, 1963; March & Simon, 1958; Simon, 1947) and the theory of agency (Jensen & Meckling, 1980; Mitnick, 1980; Moe, 1984; Ross, 1973). As in the behavioral tradition, particularly as it has been applied to bureaucratic politics by Bendor and Moe (1985), emphasis is on bounded rationality, adaptation, and dynamic process. As in the theory of agency, a central focus is on hierarchic control and its grounding in asymmetric information and conflict of interest.

The Endogenous Core

Let's begin with what can be viewed as the endogenous core of relationships: constituents file charges with the NLRB, staff members process these charges and filter out those judged unmeritorious, and the Board ultimately makes final decisions on charges constituting the filtered caseload. Each step in this three-step process is a potential cause of each of the others.[3]

1) Board members may choose between business and labor based on various criteria, such as personal ideology, legal precedent, the merits of the case, and the preferences of politicians overseeing agency performance. Members can be expected to differ and, at any rate, there is no good evidence of what criteria they actually employ. Nonetheless, the Board is much like a court in the sense that it can only decide the cases that come to it, with the types and average merit of cases making it to the Board level determined by the filing behavior of constituents and the filtering behavior of staff. These prior decisions shape the raw materials the Board has to work with in generating outcomes, and are fundamental causes of what we see as agency performance.

2) Staff members, like Board members, may apply a range of personal, legal, political, and organizational criteria in making decisions about which constituent charges are worthy of formal complaints. Also, like Board members, they are captives of constituent filing behavior, because

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[3] This three-step process is a simplification, of course, for purposes of analysis. It ignores, for example, cases that are voluntarily adjusted by disputants and those that are not appealed to the Board, and is not intended to be perfectly descriptive of the agency's internal processes. The task of future theoretical efforts, particularly those building on economic models of the litigation process (see note 5) is to take steps toward incorporating these factors. Whether for present purposes they are "too important" to exclude is an empirical matter, and, as we shall see, the data analysis supports this simplified approach.

they can only act on the kinds of charges that are actually submitted by labor and business. In addition, however, they are subordinates in a bureaucratic hierarchy: the Board (along with its general counsel[4]) is the principal, staff members are its agents, and an arsenal of organizational control mechanisms—pay, promotion, task-assignment, status—attempt to ensure that staff decisions are roughly consistent with the Board's own decisions and criteria. Control is problematic for several reasons. First, the staff and the Board doubtless have different interests in some respects—the Board, for instance, may be much more sensitive to politics, the staff more preoccupied with legal precedent and organizational routines—so there is no guarantee the staff will voluntarily behave as requested. Second, the staff can avoid control and pursue its own interests by taking advantage of a fundamental information asymmetry: only staff members are truly informed about the specifics of the vast majority of cases, particularly those that are dismissed, withdrawn, or settled by agreement. Thus, notwithstanding the availability of appeals procedures (yet another control mechanism), there is plenty of opportunity for staff to apply criteria different from those desired by the Board. Third, the Board's criteria may be only vaguely communicable anyway, with heavy reliance on specific formal decisions (anchored in specific sets of facts) to convey legal direction; if commands are often broad or ambiguous, staff can construe compliance very liberally. For all these reasons, then, the Board's efforts to control staff filtering behavior are likely to be only partially successful. Even with this proviso, however, one would certainly expect the causal relationship between staff decisions and Board decisions to be reciprocal: staff filtering shapes the raw materials of Board decisions, while Board decisions provide direction for staff filtering.

3) Firms, unions, and workers file charges for various reasons. Most of the time, they probably do so because they sincerely believe they have been the victims of unfair labor practices. They may also file charges for strategic reasons, for example, to hassle, embarrass, or impose costs on an opposing employer or union, especially during a strike or representation election. In either case, they will want to pursue grievances when they expect the benefits to outweigh the costs, and, particularly when the grievance is sincere, a major conditioning factor is the perceived probability of success. This probability is a composite of two separate probabilities: the probability that a charge will make it past the staff filter, and the probability that the filtered charge will subsequently prove successful at the Board level. In their filing decisions, constituents have every incentive to form expectations about these probabilities of success, basing their expectations (wholly or partially) on perceptions of actual staff and Board outcomes. Once again, therefore, the implication is one of reciprocal causality: constituent filing behavior is a basic determinant of staff and Board decision outcomes, but these outcomes have feedback effects that shape subsequent constituent decisions to file charges.

What we have, in other words, is a simple dynamic system in which imperfectly informed participants adapt to one another's decisions over time, yielding an array of outcomes. The Board makes final decisions for the agency, but can only act on the mix of cases actually filed by constituents that make it past the staff filter. The staff makes organizational filtering decisions, but only on the mix of cases constituents decide to file, and, in determining which charges to filter out, the staff takes its cues from Board decisions and adapts its criteria over time accordingly. Constituents decide whether and what types of cases to file by adjusting for changes in the perceived likelihood of success, and thus by adapting to perceived changes in the direction of Board and staff decisions.

All three outcomes—case filing, case filtering, and final decision—are products of the entire system of relationships. An exogenously caused change in any one component would reverberate throughout the system, causing a whole series of adjustments in all three components as they mutually adapt. Moreover, holding all exogenous effects constant, one would ordinarily expect such a system to attain an equilibrium in which the values of all three central components arrive at roughly stable values—roughly stable because the relationships and the factors underlying them (e.g., perceptions, bureaucratic control mechanisms) are subject to stochastic fluctuation.

Some Implications

I have data that allow for the construction of separate indexes measuring the extent to which labor is favored over business in the final decisions of the Board and the filtering decisions of staff members; each index is a reflection of the percentage of time labor wins at the relevant organizational level. I also have data on the number of cases filed by labor and business, although not on their objective merit. What does the framework outlined here suggest that we should expect of these measured aspects of organizational and constituent behavior?

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[4] To keep matters simple, I will assume that the role of the general counsel need not be taken explicitly into account.

Suppose the system is initially in equilibrium and that it is subsequently disturbed by an exogenous shock—for example, a change in presidential administration from Republican to Democrat—that causes the Board to shift its decisional balance in a prolabor direction. The immediate result is that labor is now more likely to win at the Board level. But this also sets off reactions among constituents and staff members, who adapt—perhaps quickly, perhaps slowly, depending on how short-sighted and poorly informed they are—to the Board's prolabor move.

Constituents, or at least some of them, eventually realize that the rules of the game have been altered at the Board level. In particular, labor realizes that certain employer actions that used to be regarded by the NLRB as legal are now sometimes regarded as unfair labor practices, and thus that the probability of winning on such cases has gone from near zero to some healthier figure. Similar prolabor shifts may occur all along the line for various kinds of employer actions, whatever the NLRB's initial probability of deciding in labor's favor. The upshot is that labor is now more likely to file charges; its general probability of winning has increased, and there are also new areas of employer conduct that invite a labor response. Following an analogous line of reasoning, we can see that business finds itself in the opposite situation: its probability of winning has declined, certain areas of labor conduct are effectively insulated from attack, and the number of charges business files against labor should accordingly decline.

With some lag, staff members adapt to the Board's new position by shifting their filtering decisions in a prolabor direction: given an opportunity to screen charges identical to those filed before the shift, they would now be more likely to go along with labor-filed charges and less likely to go along with business-filed charges. But, precisely because of the Board's prolabor shift, the mix of cases coming into the agency will be systematically different from those filed earlier. How do these two sets of filings compare from the standpoint of the staff's new criteria? The old filings were initiated in ignorance of the shift. Thus, labor assessed its chances more conservatively and was more selective in its filings than it would have been had it known about the new criteria. The result is a set of cases that, to the staff, appear on the average to be much more meritorious than usual, and this is reflected in a jump in labor's probability of success. Once labor begins to adjust to the new criteria, however, it moves away from what it now recognizes as undue conservatism and selectivity, filing a larger volume of cases which, as a whole, appear less meritorious to the staff. This is not because these new cases are abnormally low in merit, but because the old cases were abnormally high—a temporary phenomenon, due to the application of new criteria to a mix of cases premised by constituents on the old criteria. Over time (but perhaps very slowly), merit drops from this abnormally high level to some "normal" level consistent with stable staff criteria and stable labor perceptions of those criteria. Analogous reasoning, of course, applies to the filing behavior of business: old cases, based on overly optimistic perceptions of staff criteria, are abnormally low in merit according to the new criteria, and merit begins to rise toward some normal level as business adjusts to the new standards.

A prolabor shift by the Board therefore has both direct and indirect effects on staff filtering behavior. The direct effect is positive: for any given case, the staff is now more likely to side with labor. The indirect effect, mediated by constituent filing behavior, is negative: as the case mix adjusts and merit drops, the staff is less likely to side with labor because it has fewer legitimate opportunities for doing so. If we measure filtering behavior by the relative tendency of labor to win at the staff level, then the most plausible dynamic time path is one with an initial prolabor shift followed by compensating adjustments back toward some normal level.

The Board's own decisions, which initiated this chain reaction, now become part of it. It achieved its original prolabor shift by applying new criteria to cases filed by constituents and filtered by staff under presumptions that the old criteria still obtained. But as its new criteria are expressed over time in formal decisions (and otherwise), labor becomes less conservative and selective in filing cases and business becomes more so, the staff makes filtering decisions more favorable to labor, and the filtered caseload comes to represent a truer adjustment to the Board's new criteria. In the process, the average merit of labor-filed relative to business-filed cases, as filtered by the staff, declines from abnormally high levels when the system is temporarily out of synchrony to some normal level consistent with stable perceptions of the new criteria by constituents and staff. If one measures Board behavior in terms of its relative tendency to favor labor in its formal decisions, then one should see an initial prolabor shift followed by a series of "moderating" adjustments. These adjustments do not reflect true moderation, because they occur even though the Board sticks faithfully to the new prolabor criteria. They are simply behavioral adjustments to changes in the case mix. The same is true for "moderating" adjustments at the staff level. For both, therefore, prolabor criteria may sometimes underlie manifest win-rates in formal decisions that do not appear in themselves to be evidence of prolabor behavior.[5]

Exogenous Variables: The Political Authorities

As this example serves to illustrate, even a sustained exogenous shock to one endogenous variable has system-wide impacts that are realized incrementally, with the dynamic time path and new equilibrium (if there is one) generated by a series of reciprocal adjustments. In general, this is the way the regulatory system works: external forces are imposed on one or more of the endogenous components, and the system as a whole adjusts. As a practical matter, of course, this process is not easily observed, because all the endogenous variables are constantly buffeted by diverse exogenous shocks, and the system is always in a state of disequilibrium.[6] But whether there is one exogenous factor or one hundred, the basic logic of system adjustment is the same.

My focus here is on two sets of exogenous variables, one political and the other economic. Let's take the political variables first.[7] To simplify matters, I will assume that the regulatory context is a two-tiered, strictly hierarchic system: political authorities attempt to control the behavior of their immediate subordinates (the Board), and the Board in turn attempts to control its own organizational subordinates, the staff. I will also assume that the behavior of the political authorities does not directly shape constituent filing behavior. Both are more plausible than the alternatives and, in principle, are testable. Given organizational incentive structures, it seems unlikely that lower-level staff would take their cues directly from politicians, and it seems even more unlikely that constituents would premise filing decisions on the behavior of political authorities when they can directly observe and assess the behavior of the agency itself.

The direct impact of politics, then, is on the Board. Specifically, political forces derive from major governmental institutions—the presidency, the Congress, the courts—that stand in hierarchical relation to the NLRB: in the larger scheme of political authority, they are the principals and the NLRB is their agent. These principals have certain "rights," whether formally granted by law or informally recognized in practice, to bring their higher will to bear on the agency, and they have a number of control mechanisms for attempting to ensure that the agency complies. As befits a separation of powers system, however, the allocation of "rights" across principals is not always very well specified, and the several principals struggle with one another for jurisdiction and influence. Added to this is the ever-present information asymmetry: the NLRB knows far more about the content and direction of its own behavior, from the lowest-level staff investigatory decisions to formal Board decisions, than these political authorities can hope to ascertain, even should they adopt costly and extensive monitoring methods (which they rarely do). Because the Board has its own interests to pursue, both as an organization (budgets, slack, autonomy) and as a collection of individuals (career, ideology), the authorities can expect partial compliance at best.

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[5] There is a modelling literature in the "law and economics" realm focusing on the incentives of plaintiffs and defendants to settle and litigate, and on the implications of their decisions for observed judicial outcomes. The logic of this study, linking case mix and decision outcomes, does not derive from any of these models, but has much in common with them. Most relevant here are recent articles by Priest (1977, 1980; Priest & Klein, 1984), who argues, among other things, that a) the mix of cases filed will adjust to new judicial criteria, b) there will be an equilibrium win-rate for plaintiffs that is the same regardless of the prevailing judicial criteria, and c) observed win-rates in judicial decision making therefore will not reflect the influence of ideology, precedent, or other standards that may in fact be at work, and thus are not useful measures for quantitative inquiry into the causes and consequences of such standards. Applying these conclusions to governmental agencies, I agree with the first, tend to agree with the second, and disagree with the third. Justification requires reference to the details of his model and is best left to another article (but see Rizzo, 1980, for a brief, incisive critique). Instead, I will simply report that the NLRB data suggest a link between case mix and decision outcomes, but also that there are systematic influences on outcomes by political and economic forces—influences that should never have shown up if Priest is right about the meaningless of win-rates as outcome measures. These differences aside, the point to stress is that the work of Priest and others investigating the litigation process (e.g., Gould, 1973; Landes, 1971; Landes & Posner, 1979; Posner, 1973) is directly relevant to the study of public bureaucracy, and offers a very promising basis for modelling bureaucratic decision making in a way that recognized the central role of mutually adaptive adjustment.

[6] Together with bounded rationality and lagged adjustment, this is a major reason for expecting win-rate measures to reflect external influences on bureaucratic standards, contra Priest (1977, 1980; Priest & Klein, 1984).

[7] Obviously, a more complete specification of the system would recognize the likelihood of mutually adaptive adjustment between each political actor and the NLRB, and indeed among the political actors themselves. As a first step, however, it is reasonable to assume that the political authorities are exogenous. This assumption greatly simplifies what is already a complex task, and it should not bias the current estimation. The adaptive adjustments of political authorities to the agency are lagged rather than simultaneous; and, unlike measures of oversight behavior and other control mechanisms, the measures I actually use in this article for the president (simply the identity of the office-holder) and Congress (ADA scores for the relevant members of Congress) are clearly not functions of NLRB behavior anyway.

Whether a given principal succeeds in influencing NLRB performance is crucially affected by two basic factors: its control mechanisms and its commitment to put them to use. The latter is quite necessary and cannot be taken for granted. With this in mind, let's take a brief look at the different principals.

The president has impressive weapons at his disposal (see Moe, 1982). Most obviously, he makes appointments to the Board and can thereby choose individuals whose policy views are consistent with his own; although he cannot remove them from office (they serve fixed, staggered terms of five years), careful selection minimizes the importance of this restriction, as does the fact that many members leave before their terms expire. As part of the appointment power, he chooses his own chair, who in turn plays a central role in controlling internal agency resources and hiring "appropriate" staff at all levels of the organization. In addition to appointments, the president can rely upon the OMB in monitoring the NLRB's performance, shaping its budgets, and screening and modifying its legislative proproposals, and he can also rely upon an important intangible resource, presidential leadership: precisely because he occupies the office of president, many executive employees throughout government believe he has a legitimate role to play in directing administrative behavior, and, on many issues, has a right to expect their compliance. The real question of presidential influence is not whether presidents have the means to bring their policy preferences to bear, but whether they (or their administrative arms in the institutionalized presidency) are seriously committed to doing so (see, e.g., Cary, 1967; Noll, 1971). If not, presidents may rationally decide to use appointments for patronage purposes, with no regard for a member's philosophy; they may direct OMB efforts toward politically more profitable ends; and they may fail to act as executive leaders. Although commitment doubtless varies across presidents, and there is little evidence on which to base conclusions at this point, the grounding of labor-business disputes in partisan politics suggests that presidents will often have an incentive to put their control mechanisms to use in influencing NLRB performance.

Congress also has impressive control mechanisms (see McCubbins, 1984; U.S. Senate Committee of Government Operations, 1977; Weingast, 1984). Along with the president, Congress plays a central role in determining NLRB budgets and legislative authority. It must give its consent to presidential appointments, and, through investigative and oversight powers, it can monitor NLRB performance, make life difficult or easy for bureaucrats, threaten or promote bureaucratic careers, and bring media attention to the agency. Critically located congressmen are also likely to have an incentive to put these powers to use, given the partisan relevance of labor-management relations in every district. Yet these powers are wielded by various committees, subcommittees, and chairs in both Houses. Thus, some congressional actors may be highly interested in influencing the NLRB, whereas others choose to focus their resources elsewhere; some of those interested in influence may be quite conservative, others quite liberal; and, particularly as the cast of characters changes and the commitment of actors in the various institutional bodies ebbs and flows, serious control efforts may shift from one committee to the next and back again over time. Within this complicated context of competing principals, the NLRB is faced with conflicting demands and pressures, but also with opportunities to avoid compliance by shifting responsibility and playing congressional actors off against one another. The empirical evidence on all this is entirely unclear. On the one hand, overt congressional activity is hardly an illustration of raw power at work: legislative hearings on the NLRB are sporadic and usually narrow in scope, appropriations hearings focus almost entirely on workload rather than policy, and most committee members (when they show up) appear totally uninformed and uninterested. On the other hand, it may be that committees only put their control mechanisms to use overtly when the NLRB fails to comply adequately with their wishes, and that compliance is in fact the norm (McCubbins & Schwartz, 1984; Weingast, 1984).

The courts are principals of a different type (Melnick, 1983). When they choose to use it, their power can obviously be quite potent: they are unique among political authorities in having the right to nullify, alter, or embrace NLRB decisions in individual cases, and many cases do ultimately end up in the courts. Moreover, the opportunity to appeal cases to the Supreme Court lends a finality and coherence to the system of courts that Congress' system of committees clearly lacks, making it difficult for the agency to avoid compliance. Yet, unlike presidents and congressional committees, they are entirely reactive, waiting for NLRB decisions to reach them on petitions for enforcement or review. Their flexibility is therefore extremely limited. They have also shown a respect for the NLRB's right to interpret its own mandate; the agency has shifted and reshifted its policy positions many times over the years with only infrequent intervention by the courts. Thus, the courts are clearly in an excellent position to shape NLRB performance, but the evidence suggests they have not taken a leading role in imposing direction on the agency.

In sum, all political authorities have formidable bases for influencing the NLRB in desired directions, but compliance is nonetheless problematic, which results partly from institutional conditions that they can do little about: the ambiguity and competitive arrangement of governmental authority, and the NLRB's crucial informational advantage. But it also results from the fact that control mechanisms must be put to systematic use if they are to be effective, and the motivation of authorities to do so—although clearly present and sometimes strong—is itself problematic and doubtless fluctuates over time.

All things considered, it is reasonable to expect each of the three political authorities to have at least some influence on the NLRB. If we concern ourselves with impacts that are systematic and sustained, then the most plausible and easily tested expectations are those outlined below, which I assume require no elaboration.

1) A change in presidential administration from Republican to Democrat gives rise to a prolabor shift in NLRB performance, and a change from Democrat to Republican produces a probusiness shift.

2) The more liberal the chairs and members of the congressional committees with jurisdiction over the NLRB, the more prolabor its performance.

3) The greater the tendency of the courts to overturn the NLRB in favor of labor rather than business, the more prolabor the NLRB's subsequent decisions.

Exogenous Variables: The Economic Environment

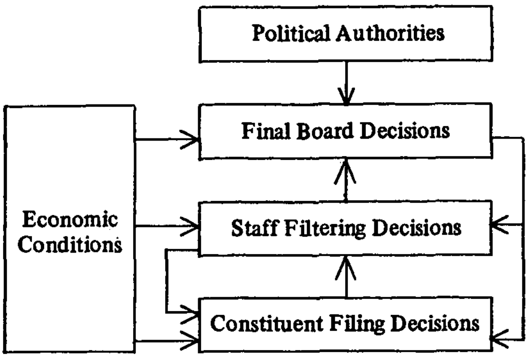
This hierarchically ordered regulatory system is embedded in a larger environment, the most relevant components of which are economic. The regulation of labor-management relations is inherently an economic task whose design, operation, and ultimate impact doubtless depend on a range of economic conditions that surround the ways in which unions, employers, and workers get along. Some of these conditions are specific to the context of labor-management relations, for example, the level of strike activity or the level of union membership. Others are general economic conditions—for example, unemployment, inflation, economic growth—that bear upon the relative strengths, economic interests, and opposing demands of labor and business, as well as on the impacts of NLRB decisions. It makes sense to think that the decisions of Board members, staff, and constituents are all sensitive in their own ways to changes in the economic environment.

But exactly how are they sensitive to these variables? This is a troublesome question that does not have an easy answer. Consider the Board, for instance. Its formal decisions clearly have an impact on the economy, and its broad mandate (supported by political pressures) clearly leads it to value stability and harmony in labor-management relations. Like the Federal Reserve Board, it may design its decision outputs in order to bring about desired economic outcomes, basing its design on an underlying model of the economy and an understanding of the impacts of its own decisions on central economic variables (Alt & Woolley, 1982; Beck, 1984). Thus, exogenous changes in economic variables—which may imply, for example, greater labor-management dispute—would call for an adaptive reaction by the NLRB. This is certainly possible. It is also possible that the agency reacts in a more political or ideological way to economic changes: it may, for example, be more sympathetic to labor during times of high unemployment. But all of this is speculation.

The same applies at the staff level. It seems reasonable to suggest that staff members do not take it upon themselves to bring their decisions into alignment with some model of the economy, and that they simply defer to the Board in this respect. Yet, they may clearly use their discretion in reacting to economic changes as they see fit—again, they may be more sympathetic to labor when unemployment is high, or they may react to high levels of strike activity or inflation in the same (or opposite) way. This sensitivity is all the more likely, in fact, given that staff members operate at the boundary of the organization, in close contact with constituents and their economic situations. Precisely what to expect, however, is unclear.

Of the three components in the endogenous core, constituents are the most directly affected by economic conditions. Strikes and representation election campaigns, for example, are obviously favorable conditions for the occurrence of unfair labor practice violations and should prompt both labor and business to file more grievances with the agency. The same may be true for a variety of other conditions—for example, unemployment and inflation—depending on their respective impacts on labor-management relations. In this article, however, my focus is on the relative filing behavior of labor and business, because this is most relevant to the decisional balance struck by the staff and the Board. Thus,

Figure 1. The Flow of Causality



the question is: What economic factors prompt labor to increase or decrease its filing of charges relative to those filed by business? In the literature on industrial strike activity, there is some evidence that unions tend to be less aggressive during times of high unemployment (see, e.g., Hibbs, 1976), so we might expect labor to file fewer cases relative to business when unemployment is high. Beyond this, however, it is unclear what to expect.

In sum, theoretical guidance on the impacts of economic conditions is of the most general sort. Given the economic nature of the NLRB's regulatory task and constituency, there is every reason to think that decision makers at all levels may adapt their behavior to changes in the economic environment, but we cannot say with confidence which variables ought to be most salient and what the direction of their impacts should be.

Data Analysis

In broad outline, then, I am proposing the causal structure of NLRB performance described in Figure 1. At its heart are the three endogenous variables—Board outcomes, staff filtering decisions, and constituent filing decisions—whose relationships with one another are reciprocal but (owing to their temporal ordering) not simultaneous. This endogenous core of relationships is embedded in the larger political and economic systems, which generate a variety of exogenous influences on the central decision components. Both within the endogenous core and between it and the external environment, important relationships are structured by a two-tiered hierarchy: politicians have authority to control the Board, and the Board has authority to control its own staff. Thus, the regulatory system is driven by exogenous shocks, mutually adaptive adjustment, and control.

In this section, I present an empirical analysis that assesses the strength and direction of these relationships and the structure of the system as a whole. Before moving to the multivariate models necessary to do so, however, I want to set the stage by introducing some very simple figures on the historical performance of the Board and its members. These figures offer an opportunity to discuss some interesting developments that have occurred over the time period, and, not coincidentally, to highlight the evidence for presidential control.

A Preface: Appointment Power and Presidential Control

The data on Board decisions (Table 1) are taken from a comprehensive coding of all (more than 12,000) NLRB formal decisions, 1948-1979, as reported in the *Labor Relations Reference Manual.* Each case involves one or more issues in which the Board is asked to decide whether the respondent is guilty or innocent of an unfair labor practice. The Board outcome is the percentage of times the majority voted guilty; the vote score for an individual member is the percentage of times he or she voted guilty. For Section 8A cases, which involve charges against employers, vote scores (labeled *VA*) are in effect prolabor indexes; the higher the score, the more frequently the Board (or individual) sides with labor. For Section 8B cases, which involve charges against unions, vote scores (*VB*) are probusiness indexes. A useful summary measure of the decisional balance between labor and business is the modified ratio, $VA/(VA + VB)$, a prolabor measure that is bounded by zero and one.[8]

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[8] Two points about this measure. First, a plausible alternative is the logarithm of the ratio VA/VB, because it also serves to mitigate the upward unboundedness problem of the simple ratio by depressing larger values. I have tried this dependent variable in the regressions to follow, and it yields results identical in all essential respects to those presented here. Second, my vote index weights all votes equally. An alternative would be to weight full Board decisions more heavily than panel decisions, in recognition that the former are often more important in terms of policy and legal precedent. Although this makes some sense, I think the unweighted index is preferable for several reasons. First, I am directly concerned with explaining who wins and who loses at the Board level, and the unweighted index is a direct measure of that. Second, the logic of the endogenous core applies to gross agency intake and output, not to a select subset, and is best tested with an unweighted measure. Third, any new principles and precedents established in full Board decisions should almost immediately shape panel decisions as well, because the same people are responsible for both. Fourth, a quanti-

Table 1 presents vote scores for all Board members, 1948-1979. Because various unmeasured forces are causing votes to fluctuate over time, it is dangerous to make intertemporal comparisons; comparisons within years are much more meaningful. With this caveat, let's take a look at those years most relevant to an evaluation of the presidential control hypothesis.[9]

We can begin with 1953, the year in which, for the first time in its history, the NLRB found itself "part" of a Republican administration. The crucial issue is: Did Eisenhower act like a partisan politician committed to shifting NLRB performance in a probusiness direction, and did his appointees behave in large degree as his agents of change? The answer on both counts appears to be positive. Eisenhower made his first two appointments in 1953, replacing two Truman appointees with Farmer (his chair) and Rogers, both of whom were far more conservative than any members of the Truman Board. Through 1957, in fact, when the last Truman holdover finally left the Board, all Eisenhower appointees consistently voted more conservatively than the remaining Truman appointees.

Two additional points are worth noting. First, it appears Eisenhower came in with guns blazing: his initial three appointees seem to be the most conservative of the bunch (Kimball, perhaps, excepted). Second, the appointment of Fanning stands out from all the others, because he is clearly a liberal—and because he was to stay on the Board for more than 20 years, longer than any other member in its history. This may have been a colossal mistake of Earl Warren proportions by a president who failed to recognize a liberal-in-the-making. It may also have been part of a second-term strategy of moderation or coalition-building—perhaps reflecting accommodation to a more liberal (and Democratically controlled) Congress, particularly the Senate Labor Committee, which must consent to all appointments.[10] Whatever the case, its legacy for the NLRB was doubtless more and different from what Eisenhower had bargained for.

In 1961, Kennedy came into office and appointed McCulloch (his chair) and Brown, who immediately established themselves as liberals and, with Fanning already on the Board, found themselves effectively in the majority. The sequence of events is again just what we would expect from a partisan change of presidential administration: the new Democratic president made liberal appointments, and the new members set about moving the Board in a prolabor direction. Upon succeeding Kennedy, Johnson made no fundamental changes; he replaced the very conservative Rodgers with the moderate Zagoria, but otherwise left the Kennedy Board intact.

Nixon did not make a quick move to transform the Board, but when he did move, he took clear steps to turn the agency in a probusiness direction; beginning with the appointment of Miller (his chair) in 1970, he appointed three conservatives in succession. All that remained for a complete transformation was the removal of Fanning and Jenkins when their terms expired. Yet Nixon chose to reappoint both of them, thereby departing from the Eisenhower-Kennedy-Johnson tradition of systematically weeding out the ideologically unacceptable holdovers from the previous administration. This change in appointing behavior did not coincide with a liberal shift in the Senate Labor Committee. Why, then, did Nixon fail to follow the transformation through? Fanning happened to be up for reappointment in 1972, an election year in which Nixon was making a special attempt to attract labor support; certain unions made it clear they wanted Fanning reappointed, and it was done.[11] As for Jenkins, an obvious possibility is that, as a black Republican, he was politically very difficult for Nixon to remove. Both cases suggest the importance of coalitional concerns. Whatever Nixon's true

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tative measure of who wins and who loses in full Board decisions is hardly a pristine measure of Board criteria anyway; to take but one illustration, a huge prolabor shift in principle may be followed by a number of minor probusiness adjustments to that principle, giving the mistaken appearance of a net probusiness shift. Still, because the weighted measure does carry a certain plausibility, I estimated all the models of this article with the use of two alternative indexes that respectively weighted full Board decisions three times and five times more heavily than panel decisions. Overall, the fit was not quite as good as with the unweighted measure and (as we should expect from the above logic) the filtered caseload variable did somewhat less well as the weights increased; but the results were only marginally different from those presented in Table 2, and they support conclusions that are identical in every essential respect.

[9] Member voting behavior on the NLRB has been explored by Delorme, Hill, and Wood (1981) and Cooke and Gautschi (1982). Both support the presidential control hypothesis, but both also suffer from important methodological weaknesses. Here, I offer a broad overview simply to describe the voting data and point out basic patterns.

[10] Reference here and below to congressional ideology is based on an appraisal of ADA scores for the 1948-1979 period. See note 13.

[11] This conjecture was offered to me, with obvious confidence, by a former Board member and longtime insider at the NLRB.

Table 1. Vote Scores: Tendency to Favor Labor over Business[a]

	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79
Herzog	539	552	530	459	492	471																										
Houston	578	614	587	476	525	487																										
Murdock	485	571	545	489	565	518	510	530	653	570																						
Reynolds	457	502	522	445																												
Gray	449	459																														
Styles			593	486	507	513																										
Peterson					552	465	461	479	646																							
Farmer						284	417	441																								
Rodgers						282	392	429	459	446	471	445	435	432	391	429																
Beeson							408																									
Leedom								461	491	488	483	466	482	508	445	452	451															
Bean									504	476	479	479	529																			
J. Jenkins										483	484	461	495	449																		
Fanning											516	512	529	572	553	522	550	502	493	496	460	517	555	570	517	574	598	658	626	608	580	582
Kimball														435																		
McCulloch														621	554	496	563	466	475	457	431	503	496									
Brown														601	580	548	581	518	529	499	511	549	619	635								
H. Jenkins																	531	477	511	478	506	511	534	510	513	536	528	567	515	546	540	546
Zagoria																		502	471	492	482	495										
Miller																							454	471	438	458	502					
Kennedy																								389	398	427	355	379				
Penello																									453	484	456	540	523	490	497	482
Murphy																												575	546	576	561	523
Walther																													428	525		
Truesdale																															566	519

[a] Score = 1000. $VA/(VA + VB)$, where VA is the percentage of votes against business in 8A cases and VB is the percentage of votes against unions in 8B cases.

motivation, he passed up his chance to transform the Board.

The Ford presidency produced another problematic appointment. Miller left the Board and, rather than appointing a conservative, Ford replaced him with the moderate to liberal Murphy. With the Senate Labor Committee's ideology remaining fairly constant from the early Nixon years, the most likely explanation is again coalitional. Murphy was the first woman ever appointed to the Board in its long history, and her philosophy may have been of less concern than the gains from political symbolism and coalition making. Ford "made up" for this liberal move by reappointing Penello and by appointing the moderate-to-conservative Walther, but the latter was little consolation, because Walther replaced the most conservative member on the Board, Kennedy. Thus, while the Board remained split, Ford succeeded only in nudging it in a prolabor direction.

When Carter came into office, therefore, he was not faced with a Board controlled by ideological partisans of the other side. However, he clearly did take steps to consolidate liberal power. Fanning, up for reappointment in 1977, was reappointed and made chairman; Truesdale (a moderate to liberal like Murphy) was appointed to replace Walther; and the Carter Board quickly took on a more prolabor look.

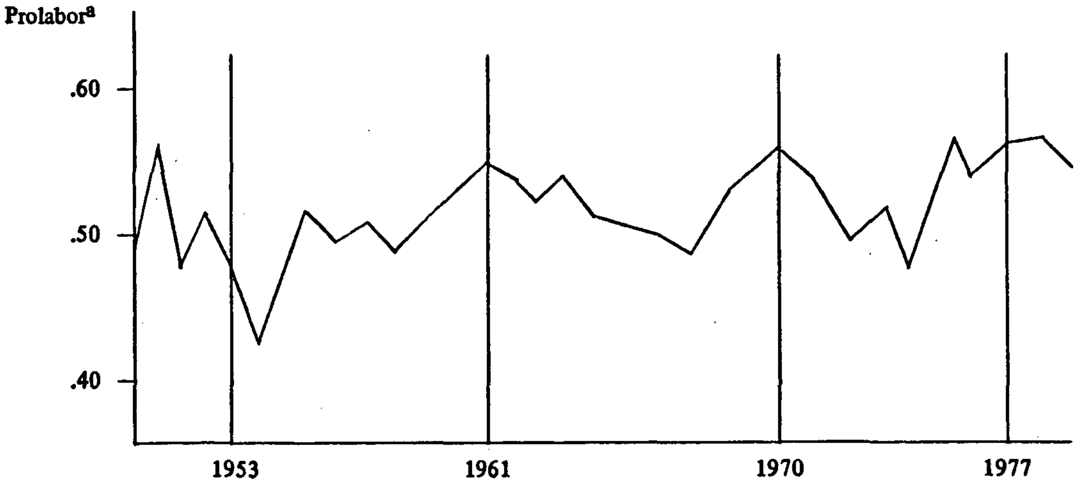
In short, the voting data suggest that presidents involved in a partisan change of administration generally behaved as the presidential control hypothesis says they should. They used their appointment power to weed out unacceptable holdovers and to bring right-thinking individuals to the Board, and to a large extent, it appears, these individuals did effectively act as presidential agents. The few exceptions all occurred during years when a Republican president faced a Democratically controlled Congress, and may well reflect presidential concern for electoral and legislative coalition building.

Presidents, however, are just one type of force on the agency, and their success in shifting aggregate performance can only be demonstrated by estimating their causal roles within the larger system of relationships. Whether the president's appointment power—as augmented by the OMB, leadership, and other presidential resources—is of significant consequence in explaining the fluctuation of NLRB behavior over time, then, remains to be seen.

Board Decisions

As Figure 2 describes, the Board's decisional balance has fluctuated quite a bit over time, but there is no long-term trend in either direction; in fact, its historical mean is exactly .5, indicating that, on average, it is equally likely to find employers and unions guilty as charged. This in itself is quite interesting, because it suggests that there may indeed be equilibrating mechanisms within the endogenous core that, subsequent to exogenous shocks, propel the central variables back toward "normal" levels. And for Board decisions, even if the criteria underlying them are decidedly prolabor or probusiness, the most obvious candidate for the equilibrium level is

Figure 2. Labor-Business Balance in Board Decisions



[a]Score = $VA/(VA + VB)$, where VA is the percentage of votes against employers in 8A cases and VB is the percentage of votes against labor in 8B cases.

exact equality. Moreover, if we look at the most easily identified exogenous shocks, partisan changes in presidential administration, we see just what we would expect. Agency performance shifts in the predicted direction—probusiness for Eisenhower and (with a lag) Nixon, prolabor for Kennedy and Carter—and each set of initial shifts is followed by equilibrating movement back toward the mean. A superficial look at the series, in fact, would imply that there is no sustained presidential impact at all.

Questions about equilibration and presidential control, as well as about the specific roles of all the remaining political, economic, and endogenous factors, require estimation of the full causal structure. I will begin by taking Board decisions as the variable to be explained, with independent variables operationalized as follows.

1) Presidential administrations are represented by dummies, which are 0 before the president assumes office and makes his first appointment to the Board, and 1 thereafter. Each presidential coefficient, then, represents a shift in the labor-business balance that the incoming president inherits from his predecessor.[12]

2) Congress is represented summarily by an ideology index, bounded by 0 and 1, which is a weighted average of the ADA scores of the chairs and members of the relevant oversight committees in the House and Senate. A positive change in the index is prolabor.[13]

3) The courts are represented by an index that compares the extent to which the NLRB is overturned in labor's favor with the extent to which it is overturned in favor of business. A positive change in the index is prolabor.[14]

4) The filtered caseload is a modified ratio of labor-filed (*FL*) to business-filed (*FB*) cases that successfully make it past the staff filter: $FL/(FL + FB)$. This variable is an interactive reflection of both constituent filing decisions and staff filtering decisions, and is a measure of the raw materials that the Board has to work with. As the filtered caseload ratio increases, the labor-to-business case mix increases—and, for reasons discussed at length earlier, this should be associated with declines in the Board's prolabor index.[15]

5) The economic variables are commonly defined. Unemployment is the rate for urban workers. Inflation is the rate of change in the consumer price index. Economic growth is the rate of change in the industrial production index. Strike activity is an additive index of two (standardized) measures: the number of workers on strike and the number of work stoppages. Union member-

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[12] An alternative would be to ignore the first appointment and allow each dummy to take effect after the president in question has been in office for a quarter or two. This recognizes that budgetary powers, leadership, and other control mechanisms may operate with some effect before the first appointment. Given when the first appointment has historically been made in each administration, however, this approach only requires significantly different measures for Johnson and Nixon. Estimation then yields results only slightly different from those in Table 2; the overall goodness of fit is not quite as high, and Nixon's coefficient and *t*-score are somewhat smaller. Thus, the choice of measure is not crucial here in shaping outcomes. I am opting for the appointments-oriented measure because, based on existing accounts of NLRB politics and history, I think it makes the most sense. The estimation seems to bear this out.

[13] The committees are the House and Senate Appropriations Subcommittees on Labor and the House and Senate Labor Committees. Mean ADA scores are constructed for each of these bodies and each chair, and, in the weighted average, the chairs are counted three times more heavily than the committees. This is arbitrary, but at least gets at the importance of committee chairs. All ADA scores were coded from *ADA World,* 1948-1979, correcting for abstentions. I should add that the degree to which ADA scores can provide reliable measures of ideology over time is open to question, given changes in the meaning of "liberalism" for the Americans for Democratic Action and the salient issues used to measure it. Nonetheless, the index clearly does pick up basic ideological shifts—as, for example, when a chairman we all "know" to be liberal is replaced by a chairman we all "know" to be a conservative—and it has been used successfully in time series analysis (see Weingast & Moran, 1983). It also seems to be the best measure available.

[14] More specifically, this index is an average of two separate indexes, both of which are plausible measures of feedback from the courts. One compares the proportion of labor victories over the agency to the proportion of business victories. The other compares the number of labor victories to the number of business victories. In each, Supreme Court cases are weighted 5, Appeals Court review cases 3, and enforcement cases 1. The data were coded from *Labor Relations Reference Manual* reports on all court cases involving the NLRB (numbering approximately 6000), 1948-1979.

[15] Quarterly figures for case intake are from the *NLRB Statistical Summary,* published by the agency. Figures reflecting the probability of success at the staff level (the proportion of cases neither dismissed nor withdrawn—see text, "Staff Filtering Decisions") are from *NLRB Annual Reports.* Because the latter are only available in annual form, I used a moving average to construct a quarterly probability measure that should be a better approximation of the true underlying quarterly figures. As defined in the text, *FL* at time t is operationalized as the number of cases filed by labor at $t-1$ multiplied by labor's estimated probability of success at the staff level at t. And similarly for *FB*. The lag reflects the fact that it takes the typical case a few months to make it to the formal complaint stage.

Table 2. Board Decisions: Tendency to Favor Labor over Business[a]

	Basic Model			Electoral Model		
Variable	Coefficient	t-score	Beta[b]	Coefficient	t-score	Beta
Constant	.477*	5.94	.621	.468*	5.68	.513
Eisenhower	−.074*	−3.04	−1.478	−.068*	−2.78	−1.353
Kennedy	.041*	2.95	.815	.032*	2.24	.635
Johnson	.020	1.32	.402	.027	1.77	.541
Nixon	−.028*	−1.96	−.563	−.024	−1.64	−.476
Ford	.018	1.06	.352	.019	1.10	.369
Carter	.053*	2.96	1.057	.053*	3.02	1.068
Congress (prolabor), t-1	.118*	2.79	.307	.135*	3.18	.352
Courts (prolabor), t-1	.011*	2.16	.192	.012*	2.47	.221
Unemployment (log), t-1	.062*	2.97	.337	.058*	2.88	.326
Inflation, t-1	−1.480*	−2.57	−.264	−1.650*	−2.80	−.294
Strike, t-1	.005	1.22	.095	.003	0.76	.059
Filtered caseload	−.150*	−2.06	−.242	−.152*	−2.07	−.245
Election–Democratic				.034*	2.07	.674
Election–Republican				−.017	−1.14	−.330
R^2	.54			.56		
$\bar{R}^2$	.49			.50		
D.W.	2.1			2.1		
N	123			123		

*$p < .05$, two-tail test.

[a] Measured as $VA/(VA + VB)$, where VA is the percentage of votes against business in 8A cases and VB is the percentage of votes against labor in 8B cases. Increase is Prolabor.

[b] What I call betas here are derived from a regression in which all nondummy variables are standardized. T-scores are unchanged.

ship is the percentage of nonagricultural workers belonging to unions.[16]

With quarterly data, it makes sense to impose lags of one period on most variables in order to allow the Board time to perceive their values and adapt to them. The filtered caseload incorporates a three-period lag, however, because the Board typically decides cases about nine months after they pass the formal complaint stage. And the presidential terms are not lagged at all, in recognition of the voting data's indication that presidential appointees make their presence felt immediately.

Regression results are set out under the Basic Model columns of Table 2.[17] The estimates provide strong support for the general linkages posited in the theoretical framework: the political authorities, economic conditions, and the filtered caseload all have statistically significant impacts on formal Board decisions. Moreover, they explain about half of the total variance—a healthy proportion, given that the series has no trend or autocorrelation and that the model is not "helped" by including lagged Board decisions on the righthand side.

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[16] Union membership is taken from the *Handbook of Labor Statistics* (Bureau of Labor Statistics), the industrial production index from *Business Conditions Digest* (Bureau of Economic Analysis), and the rest from the *Monthly Labor Review* (Bureau of Labor Statistics).

[17] All estimation has been carried out using PEC. Tables 2 and 3 are derived using its ordinary least squares procedure, *OLS*. As the Durbin-Watson statistic reported in Table 2 suggests, autocorrelation is not a problem. Although the dependent variable is bounded and therefore "limited," I have chosen to use standard regression rather than limited-dependent-variable techniques (such as logit) for several reasons. 1) The indexes I use here and in subsequent sections do not come sufficiently close to the bounds to cause problems. For values in the mid-range, the usual regression assumptions should tend to be approximated. Tests on residuals from Tables 2, 4, and 5 indicate that they are not heteroscedastic. 2) Use of regression rather than, for example, logit affords more flexibility in dealing with problems of autocorrelation and instrumental variables (see subsequent analysis). 3) Although the ratio indexes are bounded, their statistical properties are not the same as dichotomous variables or simple proportions, and one could not rely without appropriate adjustments (whatever that might entail) on the familiar limited-dependent-variable statistical packages for analysis.

Partisan changes in presidential administration had the expected impact: probusiness shifts for Eisenhower and Nixon, prolabor shifts for Kennedy and Carter. Not surprisingly, Nixon's impact is the smallest of the four. The coefficients of Johnson and Ford, who were not involved in partisan changes of administration, are insignificant but still suggestive. Very probably, a good part of the Johnson prolabor shift belongs to Kennedy, and the Ford shift is actually prolabor, arguably a consequence of his appointment of Murphy as chair.

Congress and the courts also have the expected impacts on the Board: the more prolabor the signals emanating from these political authorities, the more prolabor is the Board's decisional balance. Thus, we have been able to estimate the impacts of each of the three governmental institutions while controlling for the other two, and each accounts for a significant portion of the variance, which adds substantially to our confidence in assessing political control.

As for economic conditions, the impacts of three were strong enough to suggest that they should be kept in the model: unemployment, inflation, and strike activity. Board decisions are more prolabor the higher unemployment and strike activity and the lower inflation (although strike activity is statistically insignificant). Again, the specific mechanism underlying these effects is not known. It could have to do with an implicit model of the economy on the basis of which the NLRB adapts its decisions to engineer desired consequences. It is interesting, however, that unemployment and inflation loom above all else, and that their effects are opposite one another. In the broader political economy literature, these two economic conditions are consistently at the center of theoretical discussion about the causes and consequences of governmental performance, and it is clear that they have special anchorings in partisan politics: unemployment is particularly salient to labor and the Democrats, inflation to business and the Republicans (Alt & Chrystal, 1983; Hibbs, 1977). Thus, putting aside interaction effects between economic variables and partisanship (see below), it is possible that the Board simply accommodates pressures from both by being somewhat more sympathetic to labor in adapting to unemployment and more sympathetic to business in adapting to inflation. This would be a safe political strategy, but it is just a guess.

It is important that the filtered caseload variable has the expected negative impact, another indication that the system does indeed contain internal equilibrating mechanisms. Here we see that, as the case mix making it past the staff is increasingly weighted toward cases filed by labor—and thus, given our earlier reasoning, as the average merit of labor-filed cases declines relative to that of business—the Board's own decisions tend to shift in a probusiness direction, which establishes the first link in the equilibrating chain reaction.

The Basic Model is most useful in what it has to say about the theoretical relevance of each of these political, economic, and organizational variables. It might also be used to draw inferences about the relative importance of their impacts, but this requires that we ask much more of our estimates than simply their direction and significance levels, and we must be guarded in doing so. Multicollinearity and measurement error, in particular, can easily distort interpretations of relative impact.[18] Further, in the current model there is an additional obstacle to overcome: the usual standardization technique (using beta-weights) does not allow a meaningful direct comparison of presidential to nonpresidential effects.

All things considered, it is prudent to rest content with only the most general inferences about relative impact. We can begin by putting presidents aside for the moment, standardizing all remaining variables, and restricting attention to the latter. The results, incorporated in Table 2, suggest that the most powerful impacts within this set are associated with unemployment and Congress, for which standard deviation changes produce shifts of .307 and .337 standard deviations, respectively, in the NLRB index.

There are various ways to try to incorporate presidents into the comparison, but two seem especially useful here. First, we can simply look at each variable's incremental contribution to the model's explanatory power (the adjusted R^2), where the baseline model includes all variables but the one to be added, and where the presidential dummies are treated as a unit. The results are pre-

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[18] Multicollinearity, which makes a disentangling of partial effects difficult, doubtless influences the estimation here. If we regress each independent variable against the others in the model, the resulting R^2s (in parentheses) are as follows: Eisenhower (.95), Kennedy (.77), Johnson (.82), Nixon (.76), Ford (.72), Carter (.56), Congress (.65), courts (.47), unemployment (.67), inflation (.60), strikes (.30), filtered caseload (.70). The multiple correlations, of course, are much higher. Despite the high variances and covariances this tends to induce among the estimates, most coefficients are statistically significant from zero—but a thorough analysis would show, as well, that most of the betas in Table 2 are not significantly different from one another, and this undermines any effort to rank them in a simple manner. Measurement error is also a problem of potential consequence, particularly for the political and organizational variables. It can attenuate some estimates of impact and inflate others, thus confounding attempts to rank variables in importance.

Table 3. Comparison of Presidential to Nonpresidential Impacts

	Maximum Induced Shift in NLRB Index by Administration						Overall ΔR^2
	Eisenhower	Kennedy	Johnson	Nixon	Ford	Carter	
Presidents	−.074	.041	.020	−.028	.018	.053	.14
Congress	.061	.032	.025	.009	.006	.009	.03
Courts	.046	.022	.011	.021	.013	.011	.02
Unemployment (log)	.064	.019	.023	.015	.019	.012	.04
Inflation	−.021	−.008	−.019	−.037	−.035	−.026	.03
Strikes	.017	.009	.013	.013	.010	.012	.00
Filtered caseload	−.050	−.014	−.017	−.012	−.013	−.014	.02

sented in the rightmost column of Table 3. Although the nonpresidential variables roughly retain the rankings assigned by their betas, presidents emerge as having a far more important explanatory role (by this measure) than any other factor.

A second basis for comparison addresses the following central question: given the range of values taken on by each factor during an administration, what shift in NLRB performance would be implied by that factor's movement from the lowest to the highest values in its range? The figures are set out in the remainder of Table 3. With this interpretation of impact, the net effect of each factor varies quite a bit across administration, both absolutely and relative to those of other factors. For example, the realized impact of Congress falls over time owing to declines in the fluctuation of congressional ideology (on the relevant committees) over the period, whereas inflation, for obvious reasons, becomes a more potent force for change in the later period. The fundamental conclusion to be drawn, however, concerns the relative magnitudes of presidential and nonpresidential impacts: with only one exception (inflation during the Nixon years), a partisan change in presidential administration has always had a greater impact on NLRB performance than any other factor. Both methods of comparison, then, lead to the same conclusion.[19] Although we must be careful in generalizing, given the methodological uncertainties involved here, there is at least a reasonable basis for believing that presidents have consistently played a predominant role in shaping the directions of agency behavior.

As a final step in this section's analysis, the Basic Model can be supplemented to address the literature's political business cycle hypotheses, which asserts a relationship between elections, political pressures, and bureaucratic outcomes. The election variables are dummies, taking on the value 1 during the second and third quarters of presidential election years and 0 otherwise. The results are presented under the Electoral Model of Table 2. The Democratic coefficient is statistically significant and, although the Republican coefficient is not, neither is it so far from significant that it can be dismissed out of hand. Their directions are consistent with what most of us might expect (most of the time—Nixon's reappointment of Fanning notwithstanding): during Democratic presidential administrations, the NLRB's temporary electoral shift is in a prolabor direction, during Republican years it is in a probusiness direction. If one compares them to the presidential effects, moreover, their magnitudes are surprisingly large—which, if true, may reflect a temporary surge in presidential commitment before elections. These results seem to supply additional evidence, then, of the NLRB's responsiveness to political institutions, and particularly to presidents. But the Republican effect is questionable, and one must be careful in drawing firm conclusions.[20]

Both models presented in Table 2 are quite basic, with none of the fancier elaborations that might theoretically be justified. I explored a large number of these alternative specifications, most prominently those involving distributed lags on various righthand-side variables and those including interactions between economic conditions, presidents, and Congress. The former recognizes that the Board's memory may assign nonzero weights to variable values beyond lag one (or that some changes, once started, take time to be realized); the latter recognizes that the impact of economic conditions on the Board may depend on the signals it is getting from the president and Congress, that the impact of Congress may depend on who is president, and that the impact of presidents may depend on how liberal the relevant members of Congress are—particularly those on the Senate Labor Committee, given their formal role in the appointments process. None of these models proved successful, perhaps because of technical problems entailed in their estimation (multicollinearity, especially), but more likely because the underlying hypotheses are just incorrect. At any rate, the simple models in Table 2 are the culmination of an extensive process of model construction and estimation in which many possible specifications were entertained. It so happens that the best models turned out to be the most straightforward.

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[19] Note that these comparisons are anchored in how much each independent variable did in fact change. Had Congress experienced changes in partisan control throughout the period, as the presidency did, its impact might have been much greater (and would have been, if the same coefficient applied). Thus, although it may be quite true that presidents have been "more important" than Congress in explaining NLRB behavior, the reverse might have proved the case with greater changes in the partisanship of congressional committees and their chairs.

[20] Further analyses (allowing, among other things, for interactions with congressional ideology) show that off-year elections have no impact whatever on NLRB behavior. To the degree it exists, the electoral effect is limited to presidential election years.

Staff Filtering Decisions

The analysis of staff filtering decisions is complicated somewhat by two considerations. One is that many cases are voluntarily withdrawn before the staff can formally decide to filter them out, ordinarily because constituents anticipate dismissal. Thus, the best measure of staff behavior is not the percentage of cases formally dismissed, but rather the percentage either dismissed or withdrawn, because this is a direct measure of the effective degree of screening. The second consideration represents a greater difficulty: these data are only available in annual form, which still allows for econometric tests, of course, but reduces modelling flexibility and statistical degrees of freedom.[21]

Staff decisions are scored by a prolabor ratio, *BFAIL/(BFAIL + AFAIL)*, where *BFAIL* is the proportion of employer-filed 8B cases dismissed or withdrawn and *AFAIL* is the proportion of labor-filed 8A cases dismissed or withdrawn. As with the Board, it is of some interest that this measure fluctuates without trend over time around a mean of .49—a decisional balance of almost exact equality, which speaks, once again, to the likelihood that there are equilibrating mechanisms at work.

The basic results, presented in Table 4, in fact offer strong empirical support for this second link in the process of mutually adaptive adjustment: staff filtering decisions adapt in the expected direction to both constituent filing decisions and the Board's formal decisions. Moreover, this model explains virtually all of the variance in staff filtering (although the autocorrelation correction helps out).

The link to constituency is straightforward: the greater the relative tendency of labor to file cases, the less prolabor are the staff's filtering decisions

Table 4. Case Filtering Behavior: Tendency to Favor Labor over Business[a]

Variables[b]	Coefficient	*t*-score	Beta
Constant	.429*	9.18	
Unemployment (log)	.023*	2.72	.233
Inflation	.203*	2.50	.251
Unfiltered caseload	−.215*	−2.63	−.358
Board decisions × filtered caseload	.498*	6.44	.949
R^2	.95		
$\bar{R}^2$	.95		
ρ	−.50		
N	30		

*$p < .05$, two-tail test.

[a] Measured as *BFAIL/(BFAIL + LFAIL)*, where *BFAIL* is the percentage of cases filed by business against labor that are dismissed or withdrawn and *LFAIL* is the percentage of cases filed by labor against business that are dismissed or withdrawn. Increase is prolabor.

[b] This is an instrumental variables regression with unfiltered caseload and board decisions × filtered caseload treated as endogenous. All righthand-side variables are measured so as to reflect a six-month lag.

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[21] The analysis of Table 4 uses PEC's autocorrelation procedure (*AUTEST*) and the Fair (1970) method for incorporating instrumental variables. Given annual data, the Unfiltered Caseload and Board Decisions components are best viewed as current endogenous despite the six-month lag; and these, along with the lagged endogenous component present in Filtered Caseload, counsel use of instrumental variables. Instruments are exogenous variables in the larger theoretical model and, as required by Fair and the PEC routine, lagged values of all endogenous variables.

—a reflection, again, of the underlying merit factor. The link to the Board is less direct, because the staff cannot simply take its cues from labor's relative win-rate in formal Board decisions. This vote score for the Board is not, after all, a simple mirror of the Board's criteria, but rather a complex reflection that is adulterated by the filtered case mix on which the Board was constrained to decide. For example, a Board score of .55 is in fact "more" prolabor in what it signals about the Board's underlying criteria if it derives from a case mix with a high rather than low portion of labor-filed cases. Holding the Board's score constant, then, the staff will (in effect—we need not assume they actually make such calculation) count it as increasingly prolabor the higher the proportion of labor-filed cases in the filtered case mix. With a constant case mix, on the other hand, changes in the score are directly meaningful, and increases in the score itself are counted as prolabor. It follows that a proper test for the impact of Board decisions on staff decisions requires an interaction term, and it is this term that the model shows to be highly significant.

Of the economic conditions, only unemployment and inflation have impacts on staff filtering decisions. At the Board level these variables work in opposite directions, with unemployment favoring labor and inflation favoring business, but here they both have prolabor impacts. This is an interesting result that may reflect the underlying role of partisanship. As I have shown, the Board is politicized, and its opposing responses to unemployment and inflation seem to make sense in light of their anchorings in partisan politics. On the other hand, the staff is arguably nonpolitical to a great degree, and thus, although it is constrained to follow Board precedent in its filtering decisions, its perspective on the relevance of economic conditions may be quite different; among other things, it may see both unemployment and inflation as particularly burdensome to workers and behave more leniently in response. Once again, this is only a conjecture. But it is intriguing that Board and staff responses are different—and, more generally, it is of real interest that unemployment and inflation keep popping up, in this study and in many others, as significant influences on governmental performance.

Standardizing all variables, one can get some idea of their relative impact on staff outcomes. The resulting coefficients testify (with the usual caveats) to the overwhelming importance of hierarchical control within the organization: Board decisions are far-and-away the most important behavioral determinant, to the point that a one-standard-deviation shift by the Board produces a very close to a one-standard-deviation shift on the part of the staff. Next in importance is the unfiltered caseload, followed by unemployment and inflation, which are of roughly equal weight. The picture then, is one of a staff whose behavior is structured almost entirely by hierarchical control and the mix of cases coming into the agency, and, by comparison, is only weakly open to external economic influences.[22]

Constituency Filing Decisions

The unfiltered case mix generated by labor and business is measured as a modified ratio, *ECHT/(ECHT + UCHT)*, where *ECHT* is the number of cases filed by labor against employers and *UCHT* is the number filed by employers and workers against unions. If the endogenous core is in fact a system of mutually adaptive relationships, and if the system as a whole does indeed have homeostatic properties in adjusting to exogenous shocks, then we should find that the case mix adapts to decisions at both the staff and Board levels.[23]

For reasons just outlined, however, it is not quite true to say that constituents should adapt to the actual decisions of staff and Board, because these decisions are constrained by the prior case mixes on which they were based. What labor win-rates at these levels "really mean" for the constituent's probability of success is better reflected by interaction terms that adjust win-rates for case mix. The approach here, therefore, is the same as in the filtering model.

[22] I also explored the hypothesis that staff members are sensitive to an array of political influences, but this proved resistant to test. Because the more parsimonious model of Table 4 already explains virtually all the variance, and because (especially with annual data) there are causal relationships among the endogenous and exogenous right-hand-side variables, estimation of the larger model led to very odd results and suggested only that a true separation of effects is impossible with these data. Although regrettable, this should not be regarded as very troubling. The model in Table 4 is the most plausible theoretical specification, and it works beautifully.

[23] A focus on the case mix follows from a general concern for the NLRB's decisional balance and from use throughout of ratio measures which combine 8A and 8B cases. It would be possible to disaggregate the analysis at all levels and model both types separately, but, for conceptual and methodological reasons I will not go into, this is more complicated and less appropriate for shedding light on the basic issues at hand, especially as a first step. Disaggregation is on my agenda of future work. As part of this, I intend to explore models of the separate filing behaviors of employers, unions, and individuals. Some work along these lines has recently been carried out by Roomkin (1981). Although important differences in our approaches make it difficult to put his various findings to any real use here, my disaggregation will address them explicitly.

Table 5. Filing Behavior: Relative Tendency for Labor to File Charges[a]

Variables[b]	Coefficient	t-score	Beta
Constant	.167*	4.78	
Unemployment, t-1 (log)	−.017*	−2.47	−.118
Inflation, t-1	.506	1.78	.097
Proportion unionized, t-1	.701*	8.51	.492
Staff filtering × unfiltered caseload, t-1	.692*	8.33	.485
Board decisions × filtered caseload, t-1	.308*	6.22	.362
R^2	.83		
$\bar{R}^2$	.82		
ρ_1	.24		
ρ_2	−.36		
N	122		

$^*p < .05$, two-tail test.

[a] Measured as *ECHT/(ECHT + UCUT)*, where *ECHT* is the total charges against employers and *UCHT* is the total charges against unions.

[b] This is an instrumental variables regression, in recognition of the lagged endogenous components in board decisions × filtered caseload and staff filtering × unfiltered caseload.

It is also worth noting that labor and business are not simply interested in past staff and Board criteria, but rather in what values these criteria will assume later on, when their cases reach the appropriate organizational level. Thus, we should think of them as rational actors faced with the problem of forming expectations about the future, and we should construct our model accordingly. This complicates matters a great deal, however, for there are many ways to go about modelling expectations, and it is not at all clear which is appropriate in any given situation, much less in general (see, e.g., Abrams, Froyen, & Waud, 1980; Hibbs, 1976). I might reasonably hypothesize that they have "adaptive" expectations, that they extrapolate based on past values of the variable in question, or that they have "rational" expectations based on some political-economic causal model. All of these are perhaps worth exploring, but they are best left to another forum where they can receive the detailed development they require. For present purposes, I will simply note the theoretical role of expectations and model them in the most straightforward (and most commonly employed) way, by assuming expectations are based on the most recently available value of the variable in question.

The empirical results are presented in Table 5, which provides firm support for the hypothesis of mutually adaptive adjustment.[24] Constituent filing decisions are strongly related to both staff filtering decisions and to the Board's formal outcomes. More specifically, the relative proportion of labor-filed cases goes up as Board and staff decisions, adjusted for their case mixes, reflect the application of increasingly prolabor criteria. Thus, the final link in the endogenous chain reaction now falls neatly into place.[25]

These results also have something to say about the effects of economic conditions on the relative filing behavior of labor and business. Once again, unemployment and inflation stand out: the relative proportion of labor-filed cases is negatively related to unemployment and positively related to the inflation rate. The former may well be a reflection of findings in the literature that unions are less aggressive when unemployment is high (Hibbs, 1976), whereas the latter might result from a more aggressive pursuit of benefits and grievances when workers are burdened by inflation. Whatever the explanations—and they are, in the end, unclear—unemployment and inflation appear to be the central economic determinants of NLRB performance, shaping all three components of the endogenous core.

Another economic factor of relevance to constituency filing behavior is union membership, measured as a proportion of the nonagricultural workforce. Not surprisingly, union membership is positively related to the relative tendency of labor to file cases—which has meant, historically, that a rather continuous dropoff in the unionized proportion of the workforce has been associated with a corresponding decline in the relative frequency of case-filing by labor. Although there is every reason to expect a causal relationship here, the membership variable also serves as a trend factor of sorts whose explanatory role in the equation results from their mutual long-term declines rather than the influence of short-term membership fluctuations.

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[24] The analysis of Table 5 uses PEC's *AUTOEST* procedure and the Wallis (1967) method of instrumental variables, in recognition of the lagged endogenous components in both interaction terms. Instruments are exogenous variables in the larger theoretical model.

[25] It is interesting, moreover, that both the constituency and staff models contain negative serial correlation. This, too, may well be a reflection of (uncaptured components of) mutually adaptive adjustment, because this is the general sort of error structure such adjustment implies.

Putting this trend factor aside, it is clear from the standardized regression that the relative filing behavior of labor and business is much more strongly influenced by their relative probabilities of success at both the Board and staff levels than by economic conditions. This does not mean that unemployment and inflation, and indeed a whole range of other economic factors, have no influence on the separate filing decisions of labor and business. They may in fact be of major importance in explaining why each sector files cases with the agency. Yet, if they have much the same effects on both labor and business, as seems rather likely, they can tell us little or nothing about fluctuations in relative filing behavior. What the figures do tell us is that, of the economic conditions considered here, unemployment and inflation alone are able to discriminate between labor and business—but even these influences are far less potent at doing so than the relative probabilities of success.[26]

Conclusion

These empirical results tell us something about the performance of an important regulatory agency. The NLRB is responsive to all the major governmental institutions: the presidency, the Congress, the courts. It adapts to economic conditions, particularly the rates of unemployment and inflation. And formal outcomes are embedded in an endogenous core of relationships among key regulatory actors, whose mutually adaptive behavior lends a distinctive logic and dynamic process to this system of economic regulation.

It may be that the NLRB is a little unusual, because its bipolar constituency effectively rules out capture and provides fertile ground for politicization, but it would be a mistake to dismiss it as a special case, or even to put much emphasis on its departure from the norm. The NLRB is hardly at the center of political controversy and has not been since the early 1950s. Very few politicians pay any attention to the agency, and, when they do, they tend to be obsessed with its workload rather than its policies.[27] Aside from presidential appointments and court decisions, which are factors for any agency, there are rarely overt attempts to exert policy influence.

Moreover, the general literature on bureaucracy has long emphasized that public agencies are shaped by a variety of political, economic, and organizational factors, and that they are anchored in complex systems of relationships (see, e.g., Simon, Smithburg, & Thompson, 1950). The classic notion of the "iron triangle" is just one familiar by-product of this general perspective on bureaucracy, but it illustrates the point well: the behavior of a typical agency within the triangle is influenced by politicians and constituency groups as well as the agency's internal characteristics, and bureaucratic outcomes are generated by the whole set of relationships based on mutual benefit. From the standpoint of several decades of work on public bureaucracy, our findings on the NLRB are clearly not surprising. On the contrary, they simply confirm what students of public administration have long been saying about the determinants of bureaucratic behavior.

What is surprising is that formal analyses, while signalling an exciting new period in the study of bureaucracy, have learned so little from the literature of which they are a part. Formal models tend to be dyadic, focusing on a bureau's relationships with its constituency or Congress and ignoring the internal organization altogether. They also tend to be static, with no real consideration of dynamic processes and their importance. Quantitative empirical work is rare and tends to reflect these simple theoretical formulations. To some extent, of course, these approaches are justifiable responses to complexity. But there is too often a tendency in formal work to view simplification and rigor as justification in themselves for the kinds of models produced. Models asserted to be pathbreaking on analytical criteria may in fact be dangerously misleading and ultimately of little help in advancing our knowledge.

My view is that we should try to put simplicity and rigor to use in moving toward a more comprehensive understanding of public bureaucracy. This approach calls for a recognition that formal modelling is part of a much larger body of literature with a great deal to say about the nature and determinants of bureaucratic behavior. Modelling efforts are most likely to contribute to our knowledge when they appreciate and build upon the mass of work that has gone before—work of all types, not just other formal models.

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[26] I also carried out tests to determine if constituent filing behavior is a function of political variables. The model in Table 5 was augmented with presidential, congressional, and courts variables, but the results were uniformly miserable and clearly inferior to the original.

[27] During the 1984 election year, the NLRB attracted a bit of controversy as unions complained of its Reagan-induced conservatism and the Teamsters attempted (via a political deal with the administration) to have the Board's chairman removed. Even this modest level of public attention and controversy is exceptional.

Through an analysis of the NLRB, I have tried to move in this direction by striking a compromise between comprehensiveness and dyadic simplicity, constructing a framework that mirrors the literature's emphasis on the variety of causal factors and their systemic interconnection while imposing a simple, coherent structure. Two theoretical approaches supplied orientation, the Simon-March behavioral tradition and the newly emerging theory of agency, the former providing a suitable perspective on adaptive decision making and dynamic process and the latter encouraging recognition of hierarchical relationships and the underpinnings of control. Applied to the specific context of labor-management regulation, this framework allowed for a theoretical and empirical analysis that covered much of the same terrain so central to the broader research on bureaucracy, but with the advantage of clear logical linkages among the components and quantitative tests of validity.

The results, I hope, have helped demonstrate the promise of moving in this direction and the real need for thinking more broadly about agency behavior. Presidents cannot be ignored simply because they are difficult to incorporate in rational choice models. The partial impacts of the various political institutions and economic conditions cannot be estimated in isolation from one another. And the mutually adaptive adjustment at the heart of the regulatory system cannot be understood by assuming relationships are static and nonreciprocal. Although the framework I have used for integrating these components is not formalized, it obviously could be—and, more generally, the Simon-March and principal-agent approaches are both eminently well suited to the formal analysis of bureaucratic politics. Whatever the specific path chosen, progress awaits special efforts to impose conceptual order on the substantive complexity we ultimately want to explain, not assume away.

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Farquharson and Fenno: Sophisticated Voting and Home Style

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This article is aimed at integrating two kinds of analysis of legislators' calculations of advantage. We assume that legislators operate in two arenas, in the legislative arena itself, where their calculations of advantage concern simply their effectiveness in voting (Farquharson), and in the electoral arena, where their calculations concern the rewards for their position-taking as well as their effectiveness (Fenno). Our analysis is introduced by an interpretation of voting on the Powell amendment, 1956, when some legislators apparently voted strategically and others, equally able to do so, still did not. We then develop an expected utility model of voting that accounts for such divergent choices in terms of legislators' individual beliefs about the distribution of opinions in the legislature (Farquharson) and in their constituencies (Fenno). We conclude with an analysis of the Nash equilibria of choices to vote strategically or nonstrategically.

For the past 30 years students of positive political theory have been preoccupied with problems of collective choice. Although it was intended as a critique of and an advance upon the then-prevalent views in welfare economics, Arrow's (1951) seminal contribution, *Social Choice and Individual Values* has spawned both a large technical literature in social choice (Kelly, 1978; Sen, 1970) and a growing body of research on political choice and political institutions (Riker, 1982). Samuelson (1967) doubted "Arrow's mathematical politics" was a contribution to economics, but held it in high esteem as a benchmark study of political choice:

> If the Muse of history has its wits about it and succeeds in doing justice—two hypotheses of a somewhat romantic nature—I believe that Kenneth Arrow's name will long be remembered for a new and important insight into the permanent problem of the nature of democracy. . . . [T]he Arrow result is much more a contribution to the infant discipline of mathematical politics than to the traditional mathematical theory of welfare economics. (pp. 41-42)

Some 30 years later, rational actor models of political processes have become a staple of political analysis. It is no longer odd or controversial to stipulate motives for political agents and then to trace their implications for agent behavior and social choices. As a consequence, market and nonmarket phenomena are unified in a common framework which, in turn, serves as a promising basis for a more comprehensive social science. This promise, however, has been frustrated by the profound indeterminism in rational actor models of collective choice identified by Arrow. If tastes or values in a society display even a modest amount of diversity, then the social choices that emerge from aggregating these tastes or values through voting will not in general possess equilibrium properties; rather, any choice that emerges is inherently unstable. In the context of voting models, the characterization of this instability has reached its fullest generality in the so-called chaos theorems of McKelvey (1976, 1979), Schofield (1978), Cohen (1979), and Matthews (1980).

That majority rule is badly behaved in principle is now widely appreciated and need not be documented in detail here. What does require further examination and analysis are the rather stark premises and implicit commitments upon which this conclusion rests. The task of scrutinizing, relaxing, and refining these implicit commitments

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is one of substantial magnitude, constituting in itself a research program of major proportions. In the first part of this article we note in passing some of the work that has begun this enterprise. The main purpose of this article, however, is both less ambitious and more substantive.

We are interested in some aspects of political institutions and how they structure political choices. We are specifically interested in the political context in which such institutions and their members are embedded. In thinking about institutions in general, and legislatures in particular, we have been struck by how much in isolation they tend to be treated. For some purposes this is perfectly appropriate. Each of us knows congressional scholars who have never felt the urge to venture beyond the city limits of Washington. But Mayhew (1974) and Fiorina (1974) reminded us a decade ago that, although congressmen choose and behave in Washington, they have an eye on the folks back home. They vote, attend committee meetings, make speeches, draft bills, and do casework in the legislative arena; but they secure political support, campaign resources, and electoral rewards in the constituency arena.

Recently, Fenno (1978) elaborated the "two arenas" hypothesis in impressive fashion. In *Home Style,* he sought to escape the strictures of Washington (much as congressmen themselves do) in order to answer "one question central to the representative-constituency relationship [which] remains underdeveloped. It is: What does an elected representative see when he or she sees a constituency? And, as a natural follow-up, What consequences do these perceptions have for his or her behavior?" (Fenno, 1978, p. xiii).

We join Fenno in taking the two-arenas hypothesis seriously. As distinct from Fenno's more empirical exploration outside Washington, however, ours is explicitly more analytical. We believe that legislators are calculators of advantage in the institutional arena, but that they understand, as Fenno does, where the rewards are. Their behavior in Washington, then, is in part instrumental; it is aimed at the external, constituency arena. In turn, constituents and other interests monitor legislator performance. They ask the question attributed to Alben Barkley's farmer-constituent with two different emphases. They ask not only *"What* have you done for me lately?" but also "What have *you* done for me lately?" That is, constituents care both about legislative results and about legislator behavior.

Some years ago, an influential monograph by Robin Farquharson provided considerable insight into the idea of strategic calculation in committee and legislative voting situations. In *Theory of Voting,* Farquharson (1969) developed the idea of sophisticated voting in sequenced binary voting processes like those observed in legislatures. According to this idea, result- or outcome-oriented legislators should regard the alternatives on the floor at any stage of the voting process (but the last) not as objects of choice per se, but rather as vehicles that carry the process into the next stage of the sequence. Their behavior in any penultimate or antepenultimate stage of the sequence, therefore, is taken less as an opportunity to demonstrate preferences over the nominal alternatives before them than as an occasion to choose instrumentally with an eye to what will survive to the ultimate vote.

Herein lies the tension! In the context of the two-arenas hypothesis, in which both results and behavior are monitored and evaluated by constituents, result-oriented strategic calculation and sophisticated behavior in the legislative arena may require actions that run contrary to the nominal preferences of important constituents. Although helpful in producing a final result desired by constituents, a strategic vote, for example on some particular amendment, may nevertheless entail behaving in a manner that directly conflicts with the wishes of constituents on the amendment in question. Such actions will need to be explained by the legislator. But can he explain those actions? Fenno regards the task of "explaining Washington behavior" as one of the core components of a legislator's home style. Surely in contemplating a sophisticated move in the legislative arena, a legislator will give prior considerations to how it will play back home—and to whether or not he can explain it satisfactorily to his supporters in order to reduce the prospects that it will become a damaging campaign issue. In short, the two-arenas hypothesis induces legislators to calculate about being sophisticated.

Many models of committee and legislative choice assume legislators are either sophisticated, strategic calculators, or, alternatively, are passive revealers of their true preferences. The purpose of this article is to make this assumption a matter to theorize about. After reviewing the abstract model of majority rule (which we call the Political Model) and its limitations, we turn to a well-documented historical example of legislative strategy. This example displays instances of strategic preference revelation and at the same time poses an anomaly. Although some legislators most likely misrevealed their true preferences in strategic fashion, not all did. Stranger still, some of those agents who failed to vote strategically "should" have if they were behaving in a purely instrumental fashion to obtain their preferred outcome. In the third part we seek to explain why they did not. Our argument revolves around the idea that principals (constituents) not only induce preferences in agents (legislators) but also constrain their modes of behavior, a point developed about congressmen with impressive skill by Fenno. We seek, in short, to wed the insights of Farquharson and Fenno, of sophistication and home style.

The Political Model and Its Limitations

We begin by describing the bare bones of what we call the political model. Popularized by Downs (1957) and extensively developed in the voluminous literature on spatial models of electoral competition (Enelow & Hinich, 1984), it was actually developed in the context of nonelectoral institutions even earlier (Black & Newing, 1951). It is given its most extensive mathematical development in the work of McKelvey (1976, 1979) and Schofield (1978).

The Political Model

Let $N = \{1,2, \ldots, n\}$ be a committee or legislature consisting of n agents who must choose, by majority rule, an element of the set X (normally modeled as a multidimensional Euclidean subspace). Assume each agent has well-defined preferences over the ponts in X satisfying certain technical requirements (typically continuous and strictly convex preferences). Let P_i represent agent i's preferences (xP_iy means x is preferred by i to y). For two points x and y in X, x is said to be majority-preferred to y (xPy) if and only if

$$|xP_iy| > |yP_ix|$$

(where $|xP_iy|$ means the number of agents in N preferring x to y, and $|yP_ix|$ is defined analogously). For any point $y \epsilon X$, we may describe the points which majority-defeat it:

$$W(y) = \{x \epsilon X | xPy\}.$$

$W(y)$ is called the win set of y; it is the set of points that are preferred by a majority to y.

It is now well known that the majority preference relation P is badly behaved: in almost all circumstances (that is, for almost any distribution of agent preferences), P has no undominated elements in X. Normally, no element in X is preferred by the majority to all other elements. Taking political agents to be bundles of preferences, and P to describe the result of aggregating these preferences by majority voting, and assuming or specifying no additional mediation by institutional arrangements or practices, the "universal instability" result may be characterized in either of two ways:

1) For "almost every" configuration of preferences, $W(y) \neq \phi, \forall y \epsilon X$.
2) For any two arbitrary points, $x, y \epsilon X$, and "almost every" configuration of preferences, there exists a finite sequence $\{x, z_1, \ldots, z_m, y\}$ such that $z_1 \epsilon W(x)$, $z_i \epsilon W(z_{i-1})$ for $i=2, \ldots, m$, and $y \epsilon W(z_m)$.

The first statement asserts the generic nonemptiness of win sets: no point is invulnerable to defeat in a majority-rule contest. The second statement asserts not only that win sets are nonempty, but also that their content is sufficiently rich to permit any point to be reached, via a finite sequence of majority-rule contests, from any other point. In short, there is no equilibrium of majority tastes and, indeed, no basis for asserting priority of one alternative over another because each can be reached from the other by an appropriately chosen agenda.

The Political Model is elegant, but utterly simple. Tastes are taken as exogenous, individual agents passively reveal their tastes, any two alternatives in X may be pitted against one another and a social choice between them determined, and the victory of one alternative over another is strictly determined by counting heads. Read this list of properties to any student of Congress, and he will inquire curiously about other features of life on your home planet! Consider, specifically, three serious limitations of the Political Model.

Limitation 1. Symmetry among Agents

The Political Model—indeed, most formal models of politics—begins with a set N of participants, variously called by such names as "committee," "society," "electorate." There is a presumed symmetry (or anonymity) among political agents, each coming to the decision setting with preferences and a vote, and no other distinguishing features. What is striking about most political institutions, however, is the absence of precisely this sort of symmetry. Rarely do political institutions operate in a committee-of-the-whole mode with N an undifferentiated collection of political agents. Characteristic of most institutions is differentiation in the form of a division of labor. The U.S. Congress, for example, is characterized by political party organization, a committee system, and various leadership posts. The same is true of most other institutions—bureaucratic agencies, capitalist firms, and universities. All are characterized by structural differentiation and a division of labor.

Limitation 2. Symmetry among Alternatives

Similarly, the political model and the theorems of majority rule take as undifferentiated the set *X* of alternatives from which choices are made. The elements of *X* represent, in effect, comprehensive government programs in most applications. Yet in institutional settings we rarely observe choices posed in terms of one platform of programs versus another (indeed, this orientation is a vestigial remain of models of electoral competition (Downs, 1957)). Rather, the set *X* is typically partitioned into jurisdictions over which special kinds of property rights are assigned to organizational subunits. Thus, the undifferentiated sets *N* and *X* of our formal theories of majority rule are, in practice, collections of subsets and bundles of "rights" differentiating the agenda and choice authority of the subsets of *N* over jurisdictions in *X*.

Implicit in the undifferentiated set *X* of alternatives is the notion that any alternative may be pitted against any other alternative. Here, too, there is a presumption of symmetry (or neutrality) among alternatives that is generally not encountered in real-world institutional choice. Procedural rules combine with structural features to organize the process of social comparison and choice. Germaneness rules, the requirement that the status quo ante be voted last (i.e., a vote on final passage), privileged motions, and similar factors, combine with structural features like a jurisdictionally based committee system to destroy any presumed symmetry among alternatives.

Limitation 3. Uniformity in Behavior

Farquharson's (1969) *Theory of Voting* and a decade's worth of work in economics on incentive compatibility stimulated by the pathbreaking contributions of Gibbard (1973), Satterthwaite (1975), and Groves and Ledyard (1977), bifurcated the formal analysis of committees and legislatures. The political model and its predecessors assumed that the legislative agent was a nonstrategic revealer of his preferences. Confronted with a choice between *x* and *y*, the agent voted for the proposal standing higher in his preferences. Voting was taken to be a method by which preferences were revealed and aggregated. The vote itself was not seen as a strategic resource.

The integration of game-theoretic ideas changed all this. In making the act of voting a strategic affair, Farquharson and others exploited the fact that the process of collective choice in institutions follows a well-defined sequence. In such settings, for a legislator to make voting decisions at any stage solely on the basis of his preferences over the immediate alternatives of choice before him is an act of folly or self-indulgence. Preliminary decisions, argued Farquharson, are not merely popularity contexts because they determine the alternatives from which a social choice will emerge at the decisive ultimate stage.

Since the publication of Farquharson's monograph, most models make explicit assumptions about the behavioral mode of participants. However, even those models in which the distinction between sincere and strategic behavioral modes is appreciated typically make a blanket assumption to cover all agents, for example, "Assume all legislators engage in strategic voting." To our knowledge, this issue has always been settled by fiat. Conformity is presumed, and no effort is made to make it a matter determined endogenously.

Implications for the Political Model

Each of the three limitations of the Political Model described above involves a presumed uniformity or artificial symmetry. The empirical richness of political institutions, we claim, is found in the myriad ways in which uniformity and symmetry are violated. The Political Model has taken liberties with this richness. However, research is well underway aimed at relaxing some of the implicit premises of the political model. Structural features, especially those providing for the agenda power of committees, have been incorporated into the Political Model by Shepsle (1979, 1983), Shepsle and Weingast (1981), and Denzau and Mackay (1981, 1983). Likewise, procedural features, especially those relating to germaneness, other rules governing amendments, and the privileged status of certain motions, have also been explored by the above authors as well as by Enelow (1982), Enelow and Koehler (1980), Enelow and Hinich (1983, 1984), Blydenburg (1971), and Sullivan (1984). In all of this work, conclusions about pure majority rule drawn from the Political Model require modification or qualification. The chaos characteristic of pure majority rule need not be inherited by structurally and procedurally rich institutions based on majority rule.

The third limitation of the Political Model has not received attention, and this is what we set as our task in this article. This is not to say that a concern with sophisticated behavior has not attracted scholarly interest. Since Farquharson there have been significant contributions to questions of strategy in the context of the Political Model by Kramer (1972), McKelvey (1983), McKelvey and Niemi (1978), Miller (1980), Gretlein (1980), Shepsle and Weingast (1984), and

Banks (1984). Yet in all of these studies uniformity in behavior is presumed. Political agents are endowed with the capacity to behave strategically, but no question is asked about the *wisdom* of such behavior.

This latter question would be of no consequence if collective decision making were insulated from a wider political world. But we know that in the case of the U.S. Congress, legislators' links to external constituencies take on considerable significance (Fenno, 1978; Fiorina, 1974; Mayhew, 1974). Moreover, we know (or at least are prepared to assume here) that a legislator is judged in his constituency not only by the collective choices made by his colleagues and him, but also by the individual actions he has taken (Fiorina, 1983; Fiorina & Noll, 1978). For modelling purposes, this last point suggests that a legislator's behavioral mode—whether to behave strategically or nonstrategically in making motions and casting votes—is determined in part by his link to the constituency arena. Put differently, we can now imagine a reason for legislators to calculate in advance the wisdom of strategic ploys in the legislative arena. This in turn suggests that there may well be times when the wise legislator eschews strategy and votes in a "rationally nonstrategic" fashion.

A rational logic of choice about behavioral mode is the subject of the third part of this article. In order to motivate that analysis and to persuade the reader that it is not a theoretical sandcastle, we first present a historical example that lays bare the phenomenon of interest.

An Example and an Anomaly

One would like to describe the voting behavior of legislators in a straightforward way, perhaps with an equation the terms of which capture all its significant elements. Unfortunately, this is not easy, as we will illustrate by means of the incident now to be related. The essential problem of interpreting this event is that although it appears to involve strategic voting, one cannot be absolutely sure. Worse, however, two distinct groups of legislators had the opportunity to vote strategically, but only one apparently did so. How can one distinguish the considerations taken into account by the strategic voters from those taken into account by their nonstrategic counterparts? These are the puzzles we wish to address after we tell the story.

Our example involves a bill for federal aid to education sponsored by the Democratic leadership of the House of Representatives in 1956. The bill authorized the distribution of federal funds to the states for the purpose of building schools. The motive for this proposal was straightforward: This was ten years into the baby boom of 1946-1962, and schools were being filled up rapidly. Many state and local officials wanted help in building schools, and the Democratic leadership apparently believed it was advantageous to provide the help, or at least to make the gesture in an election year.

Several amendments were proposed on the House floor, but Adam Clayton Powell, a black congressman from Harlem, offered the crucial one. The Powell amendment provided that grants could be given only to states with schools "open to all children without regard to race in conformity with the requirements of the United States Supreme Court decisions."

The Powell amendment posed dilemmas for two kinds of Democrats. For southern Democrats, the unamended bill was attractive because it would doubtless bring more money into their districts than it would take out in taxes. But, if amended, the bill would deny aid, tax their districts for northern benefits, and threaten their institutions of racial segregation. For urban northern Democrats and Republicans with substantial numbers of black voters in their districts, the bill as amended by the Powell amendment was attractive as a symbol and as a method of diverting federal largesse to northern metropolitan areas. But, if amended, the bill promised to repel southern Democrats, whose defection would probably defeat it. In sum, northern urban representatives favored the bill a great deal if amended, and less so if not amended, while Southern Democrats liked it only if not amended. All this would have made no difference if the bill could have passed easily. But this was not the case. Many Republicans opposed the bill, not only because it increased the federal budget, but also and perhaps mainly because it increased the federal presence in local government. Consequently, the Democratic dilemma had a significant effect on the fate of the bill.

There were two relevant roll calls:

1) a vote on adding the Powell amendment, which passed, and
2) a vote on the passage of the amended bill, which failed.

The totals (including live pairs) on the two roll calls intersect as shown in Table 1. Table 1 generates four groups based on voting records (yea/yea, yea/nay, nay/yea, nay/nay), which correspond closely with identifiable natural groups based on shared political attitudes.

To interpret these attitudes, we can construct individual (and group) preference orders out of the alternatives before the House. These alter-

Table 1

Powell Amendment	Final Passage Yea	Nay	Total
Yea	132	97	229
Nay	67	130	197
Total	199	227	426

natives, the set $C = \{x,y,z\}$ of components of possible outcomes, were:

x: the bill with the Powell amendment
y: the original, unamended bill
z: the status quo, that is, no action

These alternatives can be arranged in six possible orders (*xyz, xzy, yxz, . . . , zxy*), five of which were the preference orders of natural political groups.

Consider those who voted *YY*. On the second roll call (where strategic voting was impossible), they voted for x over z. This limited their possible preference orders to *xyz, xzy,* and *yxz*. On the first roll call they voted for x over y. If they voted nonstrategically, then *yxz* is also impossible. So we eliminate *yxz* for this group, leaving *xzy* or *xyz*. We think *xzy* is highly unlikely for the Democrats. To have held *xzy* one would have wanted school aid only if the Powell amendment were adopted, that is, only if the South were deprived of aid. Considering the intensity of the Democratic leadership's commitment to the bill and the cultural norm of getting along by going along, it seems unlikely that any Democrat, except, perhaps, Powell himself, had this dog-in-the-manger preference order. Of course, some Republicans might have wanted school aid confined to the North, but even if the preferences of all of the 54 *YY* Republicans are reversed from *xyz* to *xzy* it makes no difference in this case to the outcome between y and z. For convenience, we can therefore say that all the *YY* voters had *xyz* as their ordering.

This was a natural political group, mostly northern urban, and fairly closely divided (60-40) between Democrats and Republicans. Of the 78 *YY* Democrats, 66% were from New England and the big cities of the Midwest and Middle Atlantic states (only 38% of the 227 Democrats voting on these roll calls were from these areas). Similarly, 65% of the 54 *YY* Republicans were from New England, New York, New Jersey, and Pennsylvania (whereas only 32% of the 199 Republicans voting on these roll calls were from the same states). It seems clear that the *YY* voters mainly represented northern urban districts. Presumably, Democrats and Republicans from this region were vying with each other for identification with the interests of blacks, as interpreted by Adam Clayton Powell. We will call this group the Powellians.

Those who voted *NY* clearly preferred x to z on the second roll call, where voting shows nonstrategic preferences; so, like the Powellians, they might have *xyz, xzy,* or *yxz*. Because we have eliminated *xzy* and attributed *xyz* to the Powellians, this leaves only *yxz* for the *NY* voters, if they voted nonstrategically. They also are a natural political group, the people who preferred school aid to anything else. The 42 *NY* Democrats (only 19% of the Democrats voting) are the regulars, who followed the party leadership. The 25 *NY* Republicans were presumably those (from states like Maine, Washington, and Colorado) who preferred school aid to a gesture for blacks, although they were not adamantly opposed to the gesture and preferred it to no action. We will call this group the school-aiders. Some of the school-aiders may have been "strategic Powellians"; that is, they may have shared with the Powellians the preference ordering *xyz*, but voted for y over x out of fear that the Powell Amendment would sink school aid (as indeed it did). Their strategizing (if there was any) came to naught, however, as the Powell amendment had sufficient votes without them, so it will do no harm to our analysis to include them with the genuine school-aiders.

Those who voted *NN* (107 Democrats and 23 Republicans) are clearly identifiable. They were all the southern and most of the border Democrats and Republicans, with a small scattering of northern Democrats and Republicans. Because the *NN* voters clearly preferred z to x (because they so voted on the final division, where strategic voting is impossible), it follows that their only possible preference orders were *zxy, zyx,* or *yzx*. Consider, first, *zxy*: If this were the true preference order of *NN* voters, then their nonstrategic votes would be *YN* (i.e., yea on attaching x to y). Conceivably, however, they voted *NN* strategically, while holding *zxy*. But this does not make sense for people who, as indicated by their placing z first, want most of all to defeat federal aid. To vote N on attaching the Powell amendment (i.e., for y against x) is to increase the chance of the bill ultimately passing. Surely people who place z first do not want to do that. So the preference order *zxy* is not possible for *NN* voters. At most, *NN* voters could have held *zyx* or *yzx*, both of which are compatible with nonstrategic *NN* votes. For the 116 *NN* southerners (105 Democrats and 11 Republicans), their true preferences were probably *yzx*, that is, they preferred the unamended bill (y) to the status quo (z), even though they were ultimately constrained to vote nay on final passage because the bill had been amended (*x*). A sentiment for *y* over *z* was attributed to southerners both by the House Democratic leadership and the Republicans against school aid. If the attribution was correct, then the 116 Southern *NN* voters must have had *yzx*. As for the 14 *NN* northerners (2 Democrats and 12 Republicans), they may have held either *yzx* (like the southerners) or *zyx*. If the *NN* Northerners held *zyx*, then they simply wished to oppose the Powell amendment and the bill itself, whether amended or not. If these were their opinions, it might make more sense to vote strategically (*YN*) to attach the Powell amendment, and thus make the bill more vulnerable. Still, a few persons may have found the Powell amendment so distasteful that they voted against it even though that action presumably conflicted with the most efficient attainment of their larger purpose. (Indeed, *NN* voters with *zyx* preferences were, if they existed, the exact opposite of the Powellians, who were *YY* voters with *xyz* preferences.) We think it likely that all *NN* voters, north and south, held *yzx*; but even if some northerners did not, the numbers are so small that they make no difference in this case. We will, therefore, refer to *NN* voters as southerners.

The 97 *YN* voters were all Republicans and were thus clearly identifiable with a natural political group, namely Republicans against school aid. Their preference orders may have been either of the remaining unassigned, *zxy* or *zyx*. Clearly, their first preference was to defeat the bill, but they might have had either *xy* or *yx* in the second and third places. If they had *zxy*, which means they wanted the symbolism of equal treatment for blacks but still preferred no school aid to anything else, then they voted directly in accord with their preference orderings. If, on the other hand, they had *zyx*, which means they found the symbolism of the Powell amendment less attractive than the unamended aid bill, then they voted strategically, that is, in favor of a less preferred alternative (*x*) against a more preferred alternative (*y*) at the first division in order to attain the best attainable outcome (*z*) at the final division.

It is difficult to decide between these possibilities. The issue really turns on whether or not these Republicans wanted to support desegregation symbolically. Certainly, some were, like the New England and New York Republicans, attached to traditional Republican values of sympathy with black aspirations. On the other hand, most northern blacks had by 1956 become Democrats and hence, outside the New York and New England area, very few Republicans felt obligated to black voters. Representative Bolling, a Democratic whip, put forward Harry Truman to argue the cause of the Democratic leadership. He clearly implied that he believed the Republicans were behaving strategically, which means he believed they ordered *zyx*. Of course, intensely partisan Democrats like Truman and Bolling are hardly good judges of Republican preferences. Still, no Republican troubled to deny Truman's rather maliciously stated allegations. Furthermore, some of those Republicans who voted for the Powell amendment nevertheless voted later in the session against other civil rights measures, which implies an ordering of *zyx* in this case. Without being able to prove anything numerically, we believe it is reasonable to divide the Republicans in the yea/nay category in half, 49 with *zxy* and 48 with *zyx*.

Summarizing, we have

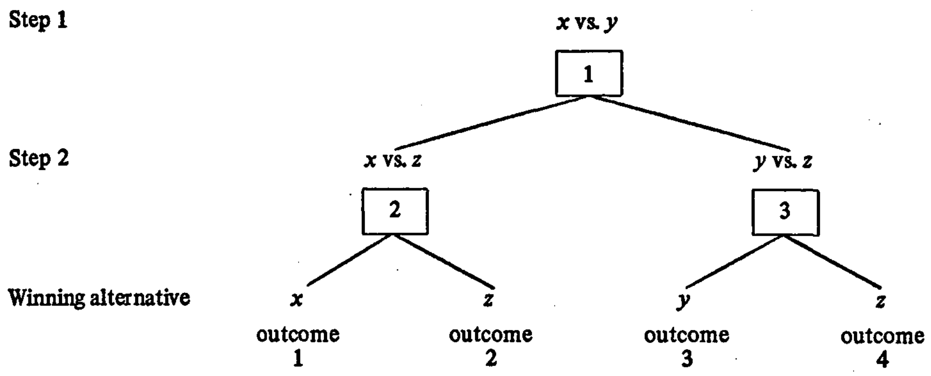
132 Powellians (78 Democrats and 54 Republicans), *YY*, *xyz*
67 school-aiders (42 Democrats and 25 Republicans), *NY*, *yxz*
130 southerners (107 Democrats and 23 Republicans), *NN*, *yzx*
49 Republicans against aid, *YN*, *zxy*
48 Republicans against aid, *YN*, *zyx*.

With this summary, it is possible to imagine ourselves into the position of congressmen before these votes were taken. In Figure 1, we present a diagram of the possible events. At step 1, the motion is to amend the bill, *y*, with the Powell amendment, *x*. Whichever survives, the amended or unamended bill, at step 2 faces the status quo, *z*. The three possible winners appear at the bottom of the diagram in four end points of possible histories (or paths).

A congressman looking forward to the vote could have seen that, if everyone voted according to the preferences previously listed (i.e., nonstrategically), then *y* would win at step 1 (see column (1) of Table 2). Then, at step 2, *y* would defeat *z* and the unamended bill would pass (column (2)). It could also have been seen, however, that those Republicans with *zyx* could defeat the bill by voting strategically (that is, as if they ordered *zxy*), so long as others continued to vote nonstrategically. This scenario is portrayed in columns (3) and (4) of Table 2. The shift of *zyx* Republican voters causes *x* to defeat *y* only subsequently to lose to *z*; that is, assuming everyone else voted nonstrategically, the *zyx* Republicans could produce victory for *z* by voting for *x* (their third choice) against *y* (their second choice). Historically, this is what happened.

But it need not have been so. From columns (1) and (2), it may be seen that *y* is a Condorcet winner, and it is well known that sophisticated voting by *all* voters yields the Condorcet winner whenever one exists. Thus, the Powellians could have countered the strategizing by the Republicans by

Figure 1. Possible Courses of Events on School-Aid Bill



voting strategically themselves, as if they had *yxz*. If they did so, they could guarantee victory for *y*, their second choice, rather than *z*, their third choice, no matter how the Republicans against aid voted at step 1. The calculations are given in columns (5) and (6), where it is apparent that no matter how the Republicans against aid voted, the outcome is *y*.

Because there is a Condorcet alternative, the winning alternative under completely nonstrategic and completely strategic voting is the same. Variations in outcome then occur, as in the case of the Powell amendment, because, for as-yet-inexplicable reasons, *only some voters voted strategically*. Here the status quo, *z*, won because just the Republicans holding *zyx* voted strategically (and, possibly some Powellians—some of those who voted *NY*—but not enough). The Powellians could have obtained their second best, rather than their worst, alternative, if they had also been willing to vote strategically. Most apparently refused to do so, *even though the situation was fully explained to them by the Democratic leadership*. Representative Bolling, a Democratic whip, obtained ex-President Truman's signature on a letter outlining the exact parliamentary situation:

> The Powell amendment raises some very difficult questions. I have no doubt that it was put forward in good faith to protect the rights of our citizens. However, it has been seized upon by the House Republican leadership, which has always been opposed to Federal aid to education, as a means of defeating Federal aid and gaining political advantage at the same time. I think it would be most unfortunate if the Congress should fall into the trap which the Republican leadership has thus set. That is what would happen if the House were to adopt the Powell amendment. The result would be that no Federal aid legislation would be passed at all, and the losers would be our children of every race and creed in every State in the Union.

Despite the persuasive explanation contained in the letter, only one black Democratic Congressman, William Dawson, the dean of the black delegation, voted with the leadership. *All the rest* and most northern urban Democrats and Republicans voted nonstrategically and thus obtained their worst outcome.

Table 2. Strategic and Nonstrategic Voting on the Powell Amendment

	Nonstrategic				Strategic by *zyx* Republicans				Fully Strategic			
	(1)		(2)		(3)		(4)		(5)		(6)	
	x vs.	*y*	*y* vs.	*z*	*x* vs.	*y*	*x* vs.	*z*	*x* vs.	*y*	*y* vs.	*z*
Powellians, *xyz*	132		132		132		132			132	132	
School aiders, *yxz*		67	67			67	67			67	67	
Southerners, *yzx*		130	130			130		130		130	130	
Republicans against aid, *zxy* or *zyx*	49	48		97	97			97	97			97
Total	181	245	329	97	229	197	199	227	97	329	329	97

This incident sets the terms of our problem. Among those who could do so, why did some vote strategically and some not? One facile, but meaningless explanation is that, for northern urbanites, *x* was worth more than the cost of *z*. But that possibility we knew all along to be true, and it does not tell us why they chose in effect to support *z* over *y* when, presumably, they valued *y* more. Why did they implicitly choose to get their worst outcome? This is an extremely difficult question to answer, especially when some similarly placed congressmen had no difficulty at all in voting strategically (some of the Republicans against aid and, perhaps, some of the Powellians). We will try to resolve the problem, however, by constructing some abstract models in which these variations make sense.

Models of Strategic Voting

To begin our analysis of this problem, assume that whenever a legislator makes a vote choice, his calculus is based on the question, "How can a potential electoral challenger use this vote against me?" As in the Powell Amendment votes, suppose a congressman can choose to vote yes (*Y*) or no (*N*) on an amendment to a bill and then yes (*Y*) or no (*N*) on either the amended or unamended bill. This admits four possible actions on his part:

YY
YN
NY
NN

Without loss of generality; assume that the outcomes are ordered *xyz* (*x*: the amended bill; *y*: the unamended bill; and *z*: the status quo) by some group in his constituency whom the congressman most wishes to please.

Adopting *xyz* as his preferences, the legislator will never vote *N* at the final division, in which either *x* or *y* is pitted against *z*, since *z* is his least-preferred alternative. This rules out *NN* and *YN* as reasonable actions. Consider now *YY* and *NY*: 1) If the congressman believes the amendment will help the bill's chance of passage on the final division, then his best action is clear—to vote nonstrategically at both divisions, *YY*. 2) If, however, he believes the amendment will hurt the bill's chances, he is faced with a more difficult choice: 2a) Suppose he votes nonstrategically *YY*, the amendment passes, and the bill is defeated (resulting in *z*, the worst outcome for his favored constituents). A challenger can now attack his vote on the amendment with a plausible counterfactual argument—"If the amendment had been defeated, then the unamended bill would have passed. Your vote on the amendment helped to defeat the bill." If the challenger can convince voters that the amendment actually did substantially lower the chance of passage of the bill, then the congressman has a lot to explain away. 2b) Suppose, instead, that the congressman votes strategically *NY*. A challenger can point at the no vote on the amendment and ask embarrassing questions about why the congressman voted contrary to the tastes of constituents who preferred *x* to *y*.

It is instructive to look somewhat more generally at the ex ante considerations a congressman may entertain concerning whether or not to cast a strategic vote. These are summarized in Table 3 where we have conjoined the two possible actions by a legislator (a strategic or nonstrategic vote on the amendment) with four possible events (the amendment passes or fails and the final bill passes or fails). In each of these eight situations, we suggest what charge (if any) a challenger might make against the incumbent's vote and what response the incumbent might offer. That is, before choosing an action, the incumbent might wish to think through Fenno's "explaining Washington behavior" problem.

What is striking about Table 3, and what sets the tone for the main point of this article, is that an incumbent congressman can get in a good deal of trouble by voting strategically. In the case of nonstrategic voting (events (i)-(iv)), only one event (iii) poses a serious "explanation problem" for the incumbent. In two cases ((i) and (ii)) there is nothing at all to explain, and in another (iv), the actual events do not substantiate the challenger's charge, and thus can be turned against him. In the case of strategic voting (events (v)-(viii)), on the other hand, every event provides something requiring explanation, and in only one case (vii) does the incumbent have a moderately persuasive response substantiated by the actual outcome. It is true that events like (vi) and (vii) permit the incumbent to demonstrate his comparative information advantage in parliamentary procedure and thereby to denigrate the inexperience and naivete of the challenger. It is also very likely that event (iii) is the worst of all possible worlds for an incumbent—one in which a favored bill goes down to defeat and the incumbent's own actions provide a smoking gun. Still, it is our view, which we believe rings true to most congressional scholars, that congressmen do not like to debate parliamentary counterfactuals with their opponents, and generally seek to avoid votes necessitating complicated explanations. In this light, strategic voting begins to look less compelling.

In the remainder of this section, we depart from previous studies of strategic voting. We explore the premise that legislators *choose* whether to behave strategically or not, and that they calculate

Table 3. Strategic Calculation: Actions, Events, Behavior

		Result of Vote		Behavior	
Action		On Amendment	On Bill	Challenger's Charge	Incumbent's Response
YY (nonstrategic)	(i)	passes	passes	none	–
	(ii)	fails	passes	none	–
	(iii)	passes	fails	You killed the bill.	No, I was behaving responsibly in voting my constituents preferences.
	(iv)	fails	fails	weaker version of (iii)	Stronger response than in (iii)
NY (strategic)	(v)	passes	passes	You misjudged in thinking amendment would hurt bill.	I was just being prudent.
	(vi)	fails	passes	The bill would have passed as amended.	No, it wouldn't have; thank God I voted against the amendment.
	(vii)	passes	fails	The bill was dead anyhow. At least you could have contributed a symbolic gesture.	The bill was not dead. The amendment killed it.
	(viii)	fails	fails	Stronger version of (vii)	Weaker response than (vii)

about this choice. They consider potential effects both on outcomes in the legislative arena and on the reputational costs of specific action-outcome pairs at home. The costs of different action-outcomes depend in part on whether the challenger can make an effective charge, and in part on whether the actual outcome will substantiate the incumbent's explanation for his behavior (i.e., was it plausible in light of the actual facts?).

We now proceed to some formal models of the legislator's decision about strategy. In the first model we treat the legislator as an expected utility maximizing decision maker. Uncertain about the choices of colleagues, he collapses those choices into "events" (e.g., amendment passes and bill fails or amendment passes and bill passes) and assigns likelihoods to them based on his subjective judgments. Our analysis yields a necessary condition for the legislator to vote strategically, and therefore provides some identification of the circumstances in which there will be a nonuniform response from legislators—some voting strategically, some in a rationally nonstrategic fashion. In the second model, we suppose that our generic legislator knows how each other legislator will vote. Given these presumed actions by others, he chooses his own optimal action. We seek to identify the behavior that emerges in equilibrium—when beliefs and optimal responses of all legislators are consistent and self-fulfilling. This is the Nash equilibrium. Our final model explores briefly equilibrium behavior when legislators are able to cooperate with one another (i.e., share information, correlate strategies, form coalitions).

Expected Utility Analysis

Let O be the set of possible outcomes,

$$O = \{AB, Ab, aB, ab\},$$

where A represents the passage of an amendment to a bill; a, its failure; B, the passage of the bill itself; and b, its failure. The possible voting choices of a legislator are in the set C:

$$C = \{YY, YN, NY, NN\}.$$

A legislator is thus faced with two separate decisions. First, he must vote on the amendment, before he knows whether the bill, amended or not, is going to pass. Second, given the division on the amendment, he must vote on the bill.

To simplify our task, we assume away the problem of identifying the group for which the legislator is agent and, without loss of generality, assume that the group he serves ranks the outcomes:

$$\begin{array}{c} AB \\ aB \\ Ab \\ ab \end{array}$$

In terms of the Powell amendment incident, $AB = x$ and $aB = y$. Note that the status quo result (z) is now partitioned into Ab and ab, distinguishing whether the amendment passes or not.

Furthermore, we assume the constituents monitor incumbent behavior as well as legislative outcomes. For any given outcome, they prefer that their representative vote for the amendment:

$$U(j, YY) > U(j, NY) \; \forall j \epsilon O$$
$$U(j, YN) > U(j, NN) \; \forall j \epsilon O.$$

That is, when it has no effect on outcomes (i.e., j is the same on each side of both inequalities), a "yes" vote is preferred by constituents to a "no" vote on the amendment. These $U(.)$ functions summarize the legislator's perception of the effect of his own actions and the legislative outcomes on his support constituency. Note that even constituents who understand and want strategic voting would hold these preferences, since the outcome, j, is held fixed across each inequality.

As noted previously, the representative has two decisions—how to vote on the amendment and then how to vote on the bill. The second decision is trivial both because the legislator's constituents unambiguously prefer him to vote for the bill and because his vote can have no strategic value at the final division. Thus, we can safely ignore actions YN and NN and focus solely on his choice of vote on the amendment, using Y and N to represent his alternatives at the first division.

Initially, we assume that he has only partial information about his colleagues but can form subjective probability estimates of how they will vote. Later, we will consider behavioral equilibrium in a richer informational setting.

In the probabilistic version, we assume that each legislator chooses his action to maximize his expected utility, $E[U(\cdot)]$. Thus, the expected utility of Y is:

$$E[U(Y)] = \sum_{j \epsilon O} p(j|Y) U(j, Y) \tag{1}$$

where Y represents voting for the amendment, $p(j|Y)$ is the probability that outcome j results when action Y is chosen, and $U(j, Y)$ is the legislator's certain evaluation of action Y if outcome j results. We define $E[U(N)]$ in a parallel fashion. The calculus for his choice is then simply whether the expected utility of Y exceeds that for N:

$$\Delta = E[U(Y)] - E[U(N)]. \tag{2}$$

This and other technical terms used below are summarized in Table 3. If $\Delta > 0$, then the legislator will rationally vote nonstrategically; otherwise he will vote strategically. We can expand Δ from equations (1) and (2) to obtain:

$$\Delta = \sum_j [p(j|Y)U(j, Y) - p(j|N)U(j, N)]. \tag{3}$$

This formulation demonstrates that the decision depends on how much $p(j|Y)$ differs from $p(j|N)$, that is, on how likely it is that a single representative's vote will change the outcome by making or breaking a tie. For the typical member in the legislature, this is a relatively unlikely event, especially if he operates noncooperatively, that is, independently of other legislators. On the other hand, if he acts in concert with a group, strategic voting may be more frequently observed (see below).

Deferring for the present a consideration of any possible cooperation, we can further analyze the determinants of strategic choice in this noncooperative context. To do so, we decompose the probability of each outcome, $p(j|Y)$ into a product of two probabilities for each of the possible outcomes $j \epsilon O$:

$$\begin{aligned} p(Ab|Y) &= p(b|A)\, p(A|Y) \\ p(Ab|Y) &= p(b|A)\, p(A|Y) \\ p(aB|Y) &= p(B|a)\, p(a|Y) \text{ and} \\ p(ab|Y) &= p(b|a)\, p(a|Y), \end{aligned} \tag{4}$$

where similar relations hold for N. The term $p(B|A)$ means the probability that the bill passes, given that the amendment passes, and the term $p(A|Y)$ means the probability that the amendment passes, given that the legislator votes Y. Note we assume that the conditional probabilities of the passage of the bill are independent of the legislator's vote on the amendment, given the outcome of the overall amendment vote. This rules out the possibility that the vote of a legislator on the bill might be influenced by the vote of another legislator on the amendment.

Note also that the probabilities must add up:

$$\begin{aligned} p(b|A) &= 1 - p(B|A), \\ p(b|a) &= 1 - p(B|a), \\ p(A|Y) &= 1 - p(a|Y), \\ p(A|N) &= 1 - p(a|N). \end{aligned} \tag{5}$$

We may write out Δ, the indicator for voting Y on the amendment, as given in equation (3):

$$\begin{aligned}\Delta = {} & p(AB|Y)\, U(AB,Y) - p(AB|N)\, U(AB,N) \\ & + p(aB|Y)\, U(aB,Y) - p(aB|N)\, U(aB,N) \\ & + p(Ab|Y)\, U(Ab,Y) - p(Ab|N)\, U(Ab,N) \\ & + p(ab|Y)\, U(ab,Y) - p(ab|N)\, U(ab,N). \qquad (6)\end{aligned}$$

Substituting the conditional probabilities given in equations (4) and (5) into equation (6), we get

$$\begin{aligned}\Delta = {} & p(B|A)\, [p(A|Y)U(AB,Y) - p(A|N)U(AB,N)] \\ & + p(B|a)\, [p(a|Y)U(aB,Y) - p(a|N)U(aB,N)] \\ & + (1-p(B|A))[p(A|Y)U(Ab,Y) \\ & - p(A|N)U(Ab,N)] \\ & + (1-p(B|a))[p(a|Y)U(ab,Y) \\ & - p(a|N)U(ab,N)]. \qquad (7)\end{aligned}$$

Voting strategically on the basis of this calculus ($\Delta < 0$) entails a tradeoff between a higher probability of one's constituents' more desired *outcome* and the reputational cost of choosing an *action* which is unpopular with constituents. A key element in this calculus is the effect the legislator's vote has on the fate of the amendment, that is, the change in the probability of the amendment passing caused by the representative changing his vote from N to Y:

$$dp = p(A|Y) - p(A|N) \geqslant 0. \qquad (8)$$

We also need a notation for the effect of the legislator's action at the first division (Y or N on the amendment) on his evaluation of the final out come:

$$e_j = U(j,Y) - U(j,N) \geqslant 0. \qquad (9)$$

Using this notation, we can rewrite Δ by substituting equations (8) and (9) into equation (7):

$$\begin{aligned}\Delta = {} & p(B|A)\, [p(A|N)\, e_{AB} + dp\, U(AB,Y)] \\ & + p(B|a)\, [(1-p(A|N))\, e_{aB} - dp\, U(aB,Y)] \\ & + (1-p(B|A))\, [p(A|N)\, e_{Ab} + dp\, U(Ab,Y)] \\ & + (1-p(B|a))\, [(1-p(A|N))\, e_{ab} \\ & - dp\, U(ab,Y)]. \qquad (10)\end{aligned}$$

All individual terms are non-negative; thus, the only terms that can yield a $\Delta < 0$ are the $-dp$ parts of the second and fourth terms of equation (10). If we group together all the dp terms, we can get a better sense of the determinants of sophisticated voting.

Let C include all the terms not involving dp:

$$\begin{aligned}C = {} & p(B|A)\, p(A|N)\, e_{AB} \\ & + p(B|a)\, (1 - p(A|N))\, e_{aB} \\ & + (1 - p(B|A))\, p(A|N)\, e_{Ab} \\ & + (1 - p(B|a))\, (1 - p(A|N))\, e_{ab}. \qquad (11)\end{aligned}$$

The term C, containing only the utility difference terms, $e_j = U(j,Y) - U(j,N)$, represents solely the expected benefits and costs of actions alone, independent of the potential effects of strategic behavior on outcomes. The remaining terms, involving dp, represent solely the potential effects of strategy, and must overcome the positive C term if there is to be rationally strategic behavior ($\Delta < 0$). Writing D as the coefficient of dp in the expression for Δ, we have from equations (10) and (11):

$$\begin{aligned}\Delta & = C + dpD \\ & = C + dp[p(B|A)\, U(AB,Y) - p(B|a)\, U(aB,Y) \\ & \quad + (1 - p(B|A))\, U(Ab,Y) \\ & \quad - (1 - p(B|a))\, U(ab,Y)]. \qquad (12)\end{aligned}$$

Using some additional notation will further simplify this expression:

$$\beta_Y = U(aB,Y) - U(ab,Y) \qquad (13)$$

(value of the unamended bill),

$$\gamma_Y = U(AB,Y) - U(aB,Y) \qquad (14)$$

(value of the amendment if the bill passes),

$$\theta_Y = U(Ab,Y) - U(ab,Y). \qquad (15)$$

(value of the amendment if the bill fails).

The last term reflects the largely symbolic value of the amendment's passage, since the bill's failure leaves the status quo unchanged.

Finally, we need to specify the effect of the amendment's failure on bill passage. We represent this difference as δ:

$$\delta = p(B|a) - p(B|A). \qquad (16)$$

Substituting equations (13) through (16) into equation (12), we can rewrite Δ in its final form:

$$\begin{aligned}\Delta = {} & C + dp\, [(1-p(B|A))\, \theta_Y + p(B|A)\, \gamma_Y \\ & - \delta\beta_Y] = C + dpD. \qquad (*)\end{aligned}$$

Because all individual terms but δ in (*) are necessarily positive, there is only one way in which Δ can be negative, that is, in which a legislator can find it rational to vote strategically on the amendment. A strategic vote is rational only if $\delta > 0$. If the amendment's failure greatly enhances the bill's chance of passage, and the unamended bill is very important to the legislator's constituents relative to the status quo ($\beta_Y > > 0$), then he may have an incentive to vote against the amendment. In doing so, he still incurs some cost owing to his vote, but this cost is now overcome by the $-dp\delta\beta_Y$ term.

Notice, however, that although $\delta > 0$ is a not very surprising necessary condition for strategic voting, it is *not* a sufficient condition. It can (and in many plausible circumstances it will) be over-

Table 4. Reader's Guide to Technical Definitions

Term	Definition	Substantive Interpretation
Δ	$E[U(Y)] - E[U(N)]$	Advantage of voting nonstrategically
$p(j\|Y)$	–	Probability of an event given the legislator votes yes on the amendment
$U(j,Y)$	–	Utility of (outcome, action) pair
dp	$p(A\|Y) - p(A\|N)$	Effect of legislator's vote on the fate of the amendment
e_j	$U(j,Y) - U(j,N)$	Evaluation of the effect of legislator's action on the final outcome
C	$p(B\|A)\,p(A\|N)e_{AB} + p(B\|a)(1-p(A\|N))e_{aB}$ $+ (1-p(B\|A))p(A\|N)e_{Ab}$ $+ (1-p(B\|a))\,(1-p(A\|N))e_{ab}$	Reputational effects of actions alone, independent of strategic effects
β_Y	$U(aB,Y) - U(ab,Y)$	Value of unamended bill relative to status quo
γ_Y	$U(AB,Y) - U(aB,Y)$	Value of amended bill relative to unamended bill
Θ_Y	$U(Ab,Y) - U(ab,Y)$	Value of amendment only relative to status quo
δ	$p(B\|a) - p(B\|A)$	Effect of amendment's failure on the final bill's prospects

whelmed by the other factors in (*). As a consequence, the decision to vote nonstrategically ($\Delta > 0$) is not an instance of myopia, naivete, or passivity in preference revelation, even though a nonstrategic vote might harm the bill's chances of final passage. It results from an instrumental calculation. Hence, we refer to it as *rationally nonstrategic* behavior.

From (*), we can assess the effect of various factors on the legislator's decision to vote strategically or rationally nonstrategically. First, the major factor in the C term is the effect on his constituents of voting against the amendment. Thus, suppose constituents themselves are so sophisticated in their thinking about the complexities of institutional politics that they care only about outcomes and not about their agent's actions, that is, $e_j = 0$ for all $j \in 0$. This implies $C = 0$, so $\Delta = dpD$. But $dp \geqslant 0$, so $\Delta < 0$ if and only if $D < 0$. From the definition of D in (*), then, $\Delta < 0$ if and only if

$$\delta\beta_Y > p(B|A)\,\gamma_Y + (1-P(B|A))\,\Theta_Y.$$

Substituting for δ, ϑ_Y, γ_Y, and θ_Y (see Table 4), the reader can see this entails a kind of expectation calculation which could go either way, depending upon the effect of the amendment on the bill's passage and the constituency's relative strength of preference as between *AB*, *Ab*, *aB*, and *ab*. Thus, even in those circumstances in which there are no "behavioral tugs" from back home (i.e., voters care only about outcomes, not agent actions), it is not always prudent for a legislator to vote strategically.

Consider two other conclusions that follow straightforwardly from (*). Suppose our legislator does not anticipate a close vote on the amendment —it will surely pass or surely fail no matter what vote he casts. Then $dp = 0$, and $\Delta = C$. That is, $\Delta > 0$ under the hypothesis, and there is nothing to be gained, and possibly much to be lost (if $e_j > 0$), from strategic voting. Similarly, suppose our legislator does not believe the fate of the bill hinges on whether the amendment passes—it will surely pass or surely fail whether amended or not. Then $\delta = 0$ and

$$\Delta = C + dp[(1-P(B|A))\,\theta_Y + P(B|A)\,\gamma_Y].$$

Again, $\Delta > 0$ under the hypothesis, and rationally nonstrategic behavior is strongly indicated.

Although additional implications of the model, particularly its comparative statics, are not developed here in much detail, the model does provide some useful insights into the problem originally posed. As equation (*) demonstrates, the decision to vote strategically or not is dependent on agent-specific beliefs and constituent-specific preferences. Consequently, we may conclude that, in an environment of probabilistic beliefs, there are circumstances in which legislative agents respond nonuniformly in matters of strategy. More generally, we have demonstrated that strategic and nonstrategic voting decisions may be treated endogenously as the product of instrumental calculation.

We now turn to an environment of richer information.

A Nash Equilibrium Analysis

In the above model we assumed that, in voting on the amendment, each legislator acted in (partial) ignorance of the votes of the other legislators. We now change that assumption and see how it affects our results. We will retain our noncooperative formulation but assume now that each legislator knows how each other legislator intends to vote; he chooses his own optimal action, given the actions of the others, that is, a reaction function. In other words, each legislator learns, through hearings, discussion, and floor debate, the vote intention of others. He needn't know each and every other legislator's plans, but only what would result in the aggregate if each acted as was their intention. He then asks the "Nash conjecture": "If everyone else acts as they intend to, should I do the same or should I act contrary to my initial intention?" If the reactions of all the agents are consistent, such that no agent wishes to change his own action given the intentions of others, then the result is a Nash equilibrium.

For simplicity, we assume that all legislators believe that the failure of the amendment is crucial to the passage of the bill. That is, they believe that the amended bill cannot pass ($p(B|A) = 0$) and that the failure of the amendment assures passage ($\delta = 1$). We show that there are three possible equilibria in such a noncooperative, but fully informed model. The key difference between this model and the expected utility model above is that each legislator knows exactly how his own behavior affects the outcome. Only if the vote is tied or is within one vote of a tie can a single legislator affect the outcome by changing his own actions. Thus, the strategic tradeoff is only relevant when the vote is "almost tied" (a plurality of no more than one vote). When the vote is *not* almost tied, the only relevant factor in the legislator's calculus is the cost of choosing the wrong action in the eyes of his constituents.

Consider again the legislator from a district which ranks the outcomes: *AB, aB, Ab, ab.* If the amendment is certain to pass or fail by more than one vote, then he has an incentive to support it (vote *Y*), as this enables him to vote his constituent's preference on the amendment without being saddled with responsibility for the amendment's fate. Because this is true for all other similarly situated legislators, one equilibrium entails none of these legislators casting a strategic vote. A legislator from a district with the opposite ranking, (*ab, Ab, aB, AB*) would have no incentive to behave strategically and would vote *N* for the same reason. If the outcome of such rationally nonstrategic voting is passage or failure of the amendment by more than one vote, then all legislators have consistent reactions and have behaved in a rationally nonstrategic fashion. Thus, one Nash equilibrium is no strategic voting.

Suppose, however, that some mixture of rationally nonstrategic and strategic voting (ranging from a circumstance in which all legislators cast a rationally nonstrategic vote to one in which all vote strategically) yields an almost-tied, *but failing* outcome on the amendment. This leaves us with the reactions of four classes of voters to consider, as shown in Table 5.

Because the initial situation, with $P(B|A) = 0$, will result in the best feasible outcome (*aB*) for the *AB* legislators (I and II), none of those already voting nonstrategically (I) has an incentive to switch to strategic behavior. Some of the strategic supporters (II), however, may have an incentive to switch from *N* to *Y* on the amendment, if the costs of behaving strategically are high enough (the *C* term), compared to the benefits of the bill passing (as represented in the *dpD* term). It is also possible, however, for none of these voters to wish to change.

The situation is different for the *ab* voters (III and IV). Some of the rationally nonstrategic voters (III) may wish to tie the vote by voting strategically. But all of the strategic voters (IV), that is, those who voted *Y* on the amendment even though they opposed it, wish to avoid the costs of behaving strategically, as it will not change the outcome. Because the amendment is already failing in spite of their strategizing, the strategic *ab* voters (IV) can switch from *Y* to *N* and improve the result for themselves. Only if there are no class IV voters could the situation be a Nash equilibrium. The symmetric case of almost-tied with the amendment passing could be a Nash equilibrium only if there were no class II voters. In addition, the case of an exact tie is a possible equilibrium. All of these almost-tied outcomes are static equilibria only. The dynamics of the reaction functions would typically move the outcome by enough votes to be outside the almost-tied range and then toward the no-strategy equilibrium. In sum, in the Nash framework it is highly unlikely

Table 5. Analysis of Legislator Reactions

	Action	
Most preferred	Rationally nonstrategic	Strategic
AB	I	II
ab	III	IV

to find those on both sides of an issue casting strategic votes and, in any event, (*) indicates that even some of the strategic voters would wish to abandon strategy if their individual contribution were inessential.

RESULT (Nash equilibrium): The only possible Nash equilibria are

1) Rationally nonstrategic—all vote nonstrategically with whatever outcome this implies;

2) Tied—some strategizing on both sides, with those behaving strategically classifiable according to (*); and

3) Almost tied—the amendment is within one vote of a tie. Two subcases would occur:

a) Amendment fails—only those who wish the bill to pass vote strategically, that is, against the amendment; or
b) Amendment passes—only those who wish the bill to fail vote strategically, that is, in favor of the amendment.

Only in the knife-edge case of ties do we expect both sides to vote strategically; the remaining equilibria involve an asymmetric choice of behavior mode (cases 3a and 3b) or no one voting strategically (case 1). In any other case, the strategic voters always have an incentive to avoid the costs of strategy by voting in a rationally nonstrategic fashion. Given the probable instability of the almost-tied cases, this analysis very strongly suggests that there will be no strategic behavior under the assumptions of the Nash model.

Cooperative Models

Clearly, one of the missing elements in the foregoing models is some form of cooperation or concerted action. In this section, we will examine some aspects of cooperation to see how they may affect the results derived. First, we consider the simplest way of bringing in concerted action—the strong Nash equilibrium.

In examining the types of actions which could "block" an alleged equilibrium, the Nash analysis allowed only reactions taken by single agents, holding the choices of all other agents fixed. The strong Nash approach allows such actions to be the result of a group of agents changing simultaneously, again while all others remain fixed. A strong Nash equilibrium is then a set of actions such that no *group* of agents has an incentive unilaterally to change their own actions, given the fixed choices of all other agents.

It is well known that any strong Nash equilibrium must also be a Nash equilibrium. Thus the strong Nash equilibria are a subset of the Nash equilibria. The strong Nash equilibria add no new strategic voting equilibria to those analyzed as ordinary Nash. The most important difference is that the equilibrium of rationally nonstrategic voting by all agents can now be blocked by a group of agents all agreeing to vote strategically. Thus, the nonstrategic outcome *might* not be a strong Nash equilibrium.

An alternative approach to modelling cooperation is to assume that individual agents are organized into political factions or parties. These parties can play two roles. First, the party may be able to publicize the necessity for strategic voting to legislative constituencies, thereby lowering the cost for each legislator of such action. This will involve the party in defending its incumbents against the charges of challengers, but doing so more credibly and at lower cost through economies of scale.

Second, the party can help to change the decision calculus in the legislature by threatening to impose sanctions on those who refuse party discipline (party as alternative constituency). This can help in two ways. The threat of sanctions itself will push the decisions of some of the legislators in favor of voting strategically. But there is also a secondary effect. As more and more similarly situated legislators vote strategically, any such incumbent will have an easier time in responding to a challenger who alleges a wrong vote on the amendment. This latter effect can be modeled as an *n*-person Prisoner's dilemma game.

Consider a typical black congressman faced with Adam Clayton Powell's amendment. The analysis we presented in the previous section suggests that the symbolic effect of the amendment alone may cause the congressman to vote for the amendment. Suppose, however, that the resulting choice on the entire bill is a close one, and the congressman believes that his constituents will benefit from the federal aid to education and perceive the importance of those benefits. He fears, however, the effect of a vote against Powell's amendment on his next reelection campaign. Without any intervention by the Democratic leadership, his situation can be modeled vis-à-vis the other black congressmen as an *n*-person noncooperative game. Assume the following simple payoff function. If all other black congressmen vote strategically (against the amendment), it is almost costless for him to vote strategically and conceivably costly not to. If all other black congressmen, however, cast rationally nonstrategic votes, then it is very costly for any black congressman to set himself apart by engaging in strategy, a fact borne out by the actual results—only the Chicago machine congressman, William Dawson, was able to support the Democratic leadership with a strategic vote. For intermediate cases, it will involve some costs for a black congressman to vote strategically because a prospective challenger could ask why the incumbent did not vote with Adam Clayton Powell, a nationally prominent civil rights leader. Powell's amendment put every black congressman, as well as every other northern liberal congressman, in a very difficult position, forcing them to weigh the possible defeat of the education bill against the likelihood of a serious challenge in the next election. We say "possible defeat" because each congressman knew that his own vote was very unlikely to affect the outcome. Voting strategically for the sake of the party can be a very costly and futile act.

How does this Prisoners' dilemma affect the Democratic leadership's problem? Once it became clear that many northern Democrats would be unable to follow the leadership, any threatened sanctions became toothless. Sanctions can have force if they only affect a few congressmen and if they are supported by most party members. Given the high rate of defection, the potential defectors soon realized that no sanctions would be imposed and simply followed their own individual interests, modelled in the expected utility or Nash equilibrium models. As both of those models predicted, the Powellian Democrats voted in a rationally nonstrategic manner, and the result feared by the Democratic leadership occurred.

Discussion

In this article we have painted with a broad brush, leaving the reader to discover the subtler detail in the bibliographic references. We have claimed that the Political Model provides a serviceable general framework for the analysis of collective choice, but we have also sought to describe a program of further research which injects some institutional richness into an otherwise structure- and procedure-free theory. Although we have not reported on it in any great detail, such research is already well under way.

One of the effects of this research program is to highlight a significant limitation of the Political Model, namely the artificial symmetries among agents and among alternatives that are some of the model's central features. Structural properties differentiate agents from one another, for example, committees and jurisdictions differentiate the proposal and veto power of agents in different policy areas. Procedural properties distinguish among alternatives, for example, some motions are privileged (like the status quo and committee bills), some are out of order (like nongermane amendments). In short, structural and procedural features, which introduce nonsymmetric moves, strategies, and decisiveness relations, characterize a much richer game form than the simple game of pure majority rule.

In the main part of this article, we suggest that an institutionally enriched political model, too, is flawed, although in a different way. It treats an institutionalized social choice process too much in isolation. We argue that embedding an institution in a broader environment strongly suggests that the behavioral mode of the legislator should likewise be freed of artificial symmetries. Assuming that legislators always vote in some fixed relationship to their preferences over outcomes—whether strategically or nonstrategically—fails to accommodate the agency relationships inherent in representation. Partly this point of view comes from empirical observation, as in the asymmetric response of legislators to the Powell amendment. But additionally, we believe it comes from theoretical reasoning about legislators as agents. In this context, the actions of legislators are signals through which constituents (and prospective challengers) monitor legislator performance. Legislators, anticipating in advance the signaling role of their public actions, incorporate these considerations into their voting calculations. Such considerations attenuate the more instrumental connection between vote and outcome. This sort of reasoning permits us to focus on situation-specific features, both inside and outside the legislature, that influence rational choices about strategic behavior. Agents need not be "naive," "myopic," or "sincere" in order to vote nonstrategically. Rather, nonstrategic voting, like strategic voting, may be the product of calculation.

In a more empirical vein, our analysis reinforces the notion of the "position-taking" legislator (Mayhew, 1974), rationally allocating resources—including his votes, but not restricted to votes alone (Fiorina & Noll, 1978). He seeks to convey an attractive image to his constituents, and to leave an impression of strength with prospective challengers in order to deter their entry. Such image-enhancing activity, which discourages strategic behavior, comes ultimately at the expense of constituent welfare. The latter's interests would be better served if their agent felt free to behave in a strategic fashion. The hyperactive monitoring of single-interest groups, as well as sunshine in the legislature aimed at illuminating legislator vote choices, only exacerbate the problem by discouraging the strategic use of resources. Finally, we note that our arguments raise serious methodological questions for roll-call analyses and interest group ratings of legislators. If there is no strategic voting, then these various methodologies (which typically take votes at face value) are subject only to already well-known caveats. If, on the other hand, there is uniform strategic voting, then roll-call analyses oblivious to this feature of behavior are called into serious question. In principle, however, roll-call methodologies could incorporate uniform strategizing. If, however, strategic behavior is nonuniform, as we believe it is on the basis of (*) and Nash equilibrium analysis, then roll-call studies are subject to severe question.

Substantively, our analysis of strategic behavior in legislatures forces attention on the relationships among agent behavior, agent rewards, and social outcomes. Agents behave in an *institutional arena,* producing social choices there, but are rewarded in an *electoral arena.* The two arenas hypothesis distinguishes between the payoffs or rewards of agents and the policy choices their behavior produces. Thus, rational behavior in the one arena, whether strategic or not, is conditioned by the outcome prospects in the other arena. Policy choices are instruments (and perhaps only by-products) of legislators' electoral gambits (a view that echoes Downs, 1957). The expected utility, Nash, and cooperative models of strategic voting are our proposed strategies of inquiry. Although only a first pass, we think it important to continue seeking to unify Fenno's concern with agent behavior and style and Farquharson's concern with agent voting calculations. Home style conditions (limits, liberates, etc.) strategic behavior. Its nonuniform effects produce asymmetric responses in legislators who might be otherwise similarly situated. It is time our models incorporated these important nonuniform effects.

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On the Possibility of Liberal Democracy

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A complex liberal version of utilitarianism is introduced which bears similarities to John Stuart Mill's moral and political philosophy and offers a promising approach to liberal democratic theory. Formally, liberal utilitarianism is represented as a type of generalized social welfare functional which maps any given set of individual vector-valued utility functions into a social ordering. The generalized social welfare functional in question relies on the experienced members of society to classify and compare the different kinds of utilities found in individual vectors; to grant and protect libertarian rights with respect to a well-defined private kind of individual utilities; and to make the interpersonal comparisons required to aggregate over each of the public kinds of individual utilities. At the same time, it is dynamic in that it implicitly comprehends many different levels of a society's moral development from a liberal perspective.

An axiomatization of the ideal liberal utilitarian social welfare functional is provided such that, on the one hand, each person has an absolute right to individual liberty over the issues within his private sphere, and, on the other hand, has an equal voice in a liberty-respecting, strongly monotonic, anonymous and neutral public choice rule operating in a distinct public sphere. The harmony of these private and public components implies the possibility of an ideal liberal democracy.

Recent critics have cast doubt not only on the feasibility of attaining liberal-democratic ideals through liberal-democratic means, but also on the very coherence or meaning of liberal-democratic ideals themselves (Davis, 1983; Dunn, 1979, pp. 28-54; Levine, 1981; Macpherson, 1977; Spragens, 1981; Wolff, 1968). Dunn's conclusion in this regard reflects the general tenor of this critical literature: "There seems every reason to doubt the possibility of a comprehensive and coherent modern philosophy of liberalism; and perhaps more disturbingly (though perhaps also consequently) there seems little reason to believe that the more attractive values of liberalism enjoy any privileged relation to the historical process" (1979, p. 54).

Recent defenders of liberalism also seem to be wary of attempts to assign a definite meaning to liberal democratic ideals. (For rare recent defenses of the idea of a liberal good life, see Barry, 1973; Galston, 1980, 1982). Defenders usually argue either that liberalism is in some sense "neutral" with respect to competing conceptions of the good life (Ackerman, 1980; Dworkin, 1978a,b; Nozick, 1974; Rawls, 1971, 1982), or, even more strongly, that liberalism is properly skeptical of the very idea of a good life in which all value conflict has been eliminated (Berlin, 1969; Williams, 1978, 1981).

The widespread dismissal of the idea of a liberal democratic good life has been more or less reinforced by the results of axiomatic social choice theory. (For recent surveys, see Plott, 1976; Riker, 1982; Sen, 1985.) The main results of social choice theory to date constitute an imposing array of formal paradoxes, of which the two best known are Arrow's (1963, 1977) general impossibility theorem and Sen's (1970, 1976, 1982, 1983) Paretian liberal impossibility theorem. Each of these theorems demonstrates that certain basic values usually thought to be characteristic of liberal democracy will come into irreconcilable conflict over an unrestricted domain of choice. The inescapable conclusion seems to be that liberal democrats must give up any utopian notion of an ideal liberal way of life. Instead, they must radically lower their sights and apparently tolerate the fact that no single morally right lifestyle can be developed by individuals (and by societies) in answer to the inevitable conflicts of incommensurable values. In effect, liberalism is reduced to a negative sense of liberty involving noninterference with a wide variety of more or less arbitrary personal (and social) moralities (or conceptions of the good life), no one of which can properly be said to be any better than any other.

It should be recognized, however, that the paradoxes of social choice theory are not necessarily purely destructive results for liberal democrats. The paradoxes can also be interpreted as guide-

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posts to the creation of a more acceptable theory of liberal democracy. In this regard, social choice theory can be used to develop a new version of liberal utilitarianism which bears affinities to J. S. Mill's thought and serves as a credible foundation for liberal democracy (Riley, 1983, 1985a-c). According to the new utilitarianism, general utility—or, more precisely, a reasonable conception of general utility, what Mill calls "utility in the largest sense" (1977c, p. 224)—is the sole ultimate norm or value of the liberal way of life.

A key formal guidepost to the development of liberal utilitarianism is provided by Sen's Paretian liberal paradox, which demonstrates that Paretianism (the democratic principle that society's choice rule should respect unanimity or consensus) is logically incompatible with respect for absolute libertarian rights, given unrestricted domain. In order to harmonize rationally the democratic value of consensus with the liberal value of liberty, therefore, it can be argued that utility in the largest sense must connote a suitable domain restriction.

As already suggested, the nature of the domain restriction inherent in liberal utilitarianism seems to be more or less implicit in Mill's moral and political philosophy. Be that as it may, it is interesting to note that critics and defenders alike of liberal democracy have traditionally condemned Mill for his failure to resolve "a number of fundamental tensions between the values to which he is fully committed" (Dunn, 1979, p. 52. See also Berlin, 1969, especially pp. l-li, 128-129, 173-206; Grote, 1870; McCloskey, 1971; Plamenatz, 1958; Stephen, 1967; Ten, 1980; and Wolff, 1968, among many others). Recently, so-called revisionist interpreters have cast doubt on the efficacy of the traditional attack (Berger, 1984; Friedman, 1966; Gray, 1979, 1981, 1983; Lyons, 1976, 1977, 1978, 1979, 1982; Ryan, 1964, 1970, 1974; and Wollheim, 1973, 1979). The new utilitarian model to be presented is also intended as a contribution to the revisionist literature on Mill.

The main body of the present essay is organized as follows. In the first section, the concept of a generalized social welfare functional (GSWFL) is introduced and the main features of the liberal (or Millian) utilitarian GSWFL are outlined. The second section makes clear that the liberal utilitarian GSWFL implicitly comprehends many different versions or levels of the utilitarian doctrine, one for each general stage of social development from a liberal perspective. The different levels include an ideal level (corresponding to the highest stage of social progress) where the utilitarian GSWFL reduces to an ideal utilitarian social welfare functional (SWFL) operating over a suitably restricted domain; and a crude or base level (corresponding to a backward or uncivilized stage of social development) where the utilitarian GSWFL collapses to the traditional Benthamite utilitarian SWFL operating over an unrestricted domain. The third section characterizes distinct public and private spheres of utility for society and for any given individual within society, and suggests a normative interpretation of interpersonal comparisons of utility such that the desirability of comparability is generally rejected within private spheres. The fourth section then shows that the ideal utilitarian SWFL can be axiomatized such that, on the one hand, individuals and voluntary groups have absolute rights to liberty over issues within their respective private spheres, and, on the other hand, the people have democratic authority in the form of an anonymous, neutral, strongly Paretian, liberty-respecting public choice rule operating over the issues in a distinct public sphere. The fifth section concludes the essay with some brief comments on the nature of the liberal utilitarian conception of the good; some implications for the design of liberal democratic political institutions; and the obstacles in the way of any society's transition to an ideal liberal democracy.

The Liberal Utilitarian GSWFL

Define a GSWFL as a collective choice rule $F(\cdot)$ which maps any given set or n-tuple of vector-valued individual utility functions $[V_i]$ into a social preference ordering R (where R is a complete, reflexive, and transitive binary relation defined over the feasible set X of social states):

$$R = F([V_i]). \tag{1}$$

Note that the more familiar social welfare functional (SWFL) concept introduced by Sen (1970) is a special case of equation (1), that is, the case where individual utility functions are real-valued. In turn, the social welfare function (SWF) concept introduced by Arrow (1963) is a special case of Sen's social welfare functional, that is, the case where individual utility functions are ordinal and noncomparable.

According to the vector view, the utility associated with any state x is a vector of various coexisting kinds or qualities of utility:

$$\forall x\colon V(x) = (U_A(x), U_B(x), \ldots, U_K(x)), \tag{2}$$

where $A, B, \ldots, K$ denote k distinct kinds of utility value, k finite.

As Sen explains, "The vector view of utility is . . . concerned . . . with the existence of many coexisting aspects of utility. . . . The vector view can be adopted even within any one interpretation of utility, e.g., pleasure or desire. Desire, for example, can be distinguished between types, or discriminated in some other way, giving us a vector view of utility within that one interpretation of utility as desire fulfillment" (1981, p. 194). Each of the k kinds of utility value at x can be assumed to be incommensurable with the other k-1 kinds at x in terms of intensity or quantity of utility (independently of the measurability and comparability assumptions imposed within any given kind).

It is important to emphasize that the utility vector $V(\cdot)$ at any state x is related to the description of x itself. The vector form is not some arbitrary, merely subjective device whose definition varies at the whim of every individual. Instead, each kind of utility included in the vector is matched to certain features of x which are associated with that kind of utility. For example, a private kind of utility value might be assigned to x in the context of the binary choice over (x,y). This private kind of utility must correspond to some private feature of social states (e.g., person i's body) which is different in x from in y, *ceteris paribus*. There are various issues surrounding the matching of different utility kinds to respective features of social states, but for the moment I simply assume that the utility vector can be defined on the basis of a suitably enriched description of social states (possibly including the thoughts and beliefs of agents).

The essential features of the vector view to keep in mind (without going into too many technical details for present purposes) are that: there are as many elements in the utility vector at any state x as there are kinds of utilities (for example, pleasures or desires); a particular kind of utility value is assigned to x in the context of a pairwise choice over (x,y) so that any given utility kind corresponds to a well-defined sphere of pairs; several kinds of utility values can be assigned to x simultaneously in the context of the same choice over (x,y), in which case that binary choice involves several co-existing issues (the relevant spheres of pairs corresponding to the several utility kinds obviously overlap at (x,y)); and if a particular utility kind U_A is not involved in the choice over (x,y), in the sense that x does not differ from y in respect of some feature associated with that kind of utility, then it is assumed that $U_A(x) = U_A(y)$, that is, the utility vector does not distinguish between x and y in terms of that kind of utility. (For further discussion of the vector view, see Sen, 1981; Riley, 1985a, chap. 5).

A GSWFL yields a social ordering from any given n-tuple of individual vector-valued utility functions by, first, aggregating individual utility values of the same kind for each of the different kinds of utility, and second, bringing in some additional qualitative judgments to compare the different kinds which are assumed to be noncomparable in terms of intensity. Many functional forms are possible for a GSWFL. For present purposes, attention is confined to the so-called Millian or liberal utilitarian GSWFL. In this regard, it is useful to reconsider how Mill proposes to aggregate utilities of the same kind, and how he proposes to make qualitative comparisons of the various heterogeneous utilities.

Turning to the latter question first, it is clear that Mill does distinguish among different kinds or qualities of utility, and that he does think some qualities are more desirable than others whatever their relative intensities (Mill, 1969b, pp. 210-214. See also Sen, 1981, especially pp. 194-197). In Mill's view, "It is quite compatible with the principle of utility to recognize the fact, that some *kinds* of pleasure are more valuable than others" (1969b, p. 211). He seems to identify at least four kinds of utility: two private kinds (merely expedient and aesthetic utilities) and two public kinds (merely expedient and moral utilities, with moral utilities further subdivided into utilities of charity and justice); and a corresponding plurality of disutilities. (For further discussion, see Riley, 1985b, chap. 5.) Moreover, he argues that the comparison of these different kinds in terms of their desirability must be based on actual experience of the kinds in question:

> What means are there of determining which is the acutest of two pains, or the intensest of two pleasurable sensations, except the general suffrage of those who are familiar with both? . . . What is there to decide whether a particular pleasure is worth purchasing at the cost of a particular pain, except the feelings and judgement of the experienced? When, therefore, those feelings and judgement declare the pleasures derived from the higher faculties to be preferable in kind, apart from the question of intensity, to those of which the animal nature, disjoined from the higher faculties, is susceptible, they are entitled on this subject to the same regard. (1969b, p. 213)

Mill claims, "It is an unquestionable fact that those who are equally acquainted with, and equally capable of appreciating and enjoying both, do give a marked preference to the manner of existence which employs their higher faculties" (1969b, p. 211). In other words, if any person becomes aware of his "higher faculties" by experiencing and (through practice) improving their operation, then he *will* feel the desire to "assign to the pleasures of the intellect, of the feelings and imagination, and of the moral sentiments, a much higher value as pleasures than . . . those of mere sensation" (1969b, p. 211). Moral and aesthetic utilities are felt by such experienced persons to be of a higher quality than merely expedient utilities (and, of course, all disutilities), and are thought to be more desirable components of personal happiness.

According to Mill, then, it is so-called experienced persons who identify different kinds of public and private utility values at any given social state *x*, and who judge which of the kinds are more desirable parts of happiness than others. To say that experienced persons judge one kind of utility value to be more desirable than another means that they feel an incomparably stronger preference for the one kind of pleasure rather than the other. In this regard, it seems that Mill's so-called doctrine of the higher pleasures can be summarized as follows. For public kinds of utility, experienced persons prefer moral utilities of justice to charitable utilities; charitable utilities to merely expedient ones; and expedient utilities to disutilities. For private kinds of utility, aesthetic utilities are thought to be more desirable than merely expedient ones, and expedient utilities more desirable than disutilities. At the same time, experienced persons think that utilities of justice justify interfering with any private kind of utility (even aesthetic utilities). Unless justice demands such interference, however, private utilities (even private disutilities) are thought to be more desirable than public utilities (including charitable ones). Some persons will, of course, fail to exhibit the preferences that would have been held by experienced persons. The inexperienced persons exhibit a deficiency either of intellect or of will; that is, they are persons who generally make incorrect inferences about the kinds of choices they face, or suffer from an incapacity to choose as they know they should desire to choose.

It is commonly argued that Mill's appeal to the qualitative judgments of the experienced members of society is unacceptably elitist, and perhaps little more than a defense of middle-class values. (See, for example, Duncan, 1973.) But one must be careful here. Mill's hypothesis is that the various kinds of utilities (including the higher kinds) are facts of human nature which can be discovered or experienced by the vast majority of human adults (i.e., all persons "capable of rational persuasion"), if they wish. (For Mill's theory of natural kinds, see Mill, 1974, pp. 118-132, 578-590, 698-732.)[1] Mill thinks that most persons can experience the higher aesthetic and moral pleasures, if only they will exercise (and thereby improve through practice) the natural powers of will and of intellect that they inherit at birth (Mill, 1974, pp. 733-739; 1979, pp. 437-462). It must be admitted that persons who do not (or, in cases of mental defect, who cannot) exercise and develop their natural capacities, will not experience the higher pleasures. Perhaps many persons will fail to improve themselves in this sense, in which case Mill's appeal to the judgments of the experienced (i.e., those persons who have developed their natural powers of intellect and will) takes on a distinctly inegalitarian air. But there seems to be nothing unacceptably inegalitarian in principle about the Millian position, as long as inexperienced individuals themselves are free to make the inequality vanish by exercising their own natural powers. Note that the failure of inexperienced persons to feel the higher pleasures in no way invalidates Mill's hypothesis that these pleasures (as much as the lower animal pleasures apparently felt by everyone) are facts of human nature. For Mill, it is crucial that most any person can develop his given natural powers of intellect and will and thereby come to discover the higher pleasures that are latent in his own nature. It is true that this process of self-development is subject in part to external circumstances (including inherited capacities) beyond the individual's control: it only takes place (if at all) according to certain causal laws (not known with any precision) which Mill would summarize as theorems of what he calls ethology, the science of character development (1974, pp. 844-874). Nevertheless, the point is that every person's original capacities of feeling and acting are not fixed and unchangeable. Instead, through deliberate trial and error ("experiments of living") and imitation of third parties ("teachers"), he can discover the great variety of higher pleasures which his "astonishingly pliant" nature is capable of, and then design his character as if it were a work of art (Mill, 1977c, pp. 260-275). Mill's hypothesis is that if any person does expand his natural powers of intellect and will sufficiently, then that person will be capable of recognizing (by introspection) that he really does feel certain moral and aesthetic kinds of pleasures to be incomparably more desirable than others for his happiness.

The so-called doctrine of the higher pleasures is thus a set of psychological propositions about the actual utilities of any experienced person. Once these propositions are established as psychological truths and summarized as theorems of human nature, general normative principles can be framed on the basis of them, to guide any person in attaining what the experienced among us agree is desirable. Ultimately, what experienced persons really do feel is made to be the basis of what all of us should feel. Note that Mill is positing a relation between idealism and positivism insofar as his prescription of what any person ought to desire is based on a description of what any experienced person actually does desire. "Utility in the largest sense" is ideal for Mill because (and only because) he assumes there is positive evidence that most (if not all) experienced persons actually desire such an ideal conception of happiness.

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[1] According to Mill's phenomenalist epistemology, kinds are not categories imposed arbitrarily on reality by the mind according to its own indecipherable laws; instead, the "facts" or properties of things (in this case, mental states) themselves suggest their own classification to the mind according to laws which can in principle be deciphered by a true science of psychology. Generally, see Mill (1974, 1979).

As suggested above, Mill proposes a set of normative principles which he claims guides any experienced person's assignment of utility values over social states, and should guide every person's assignment of utility values. These normative principles, which can be referred to as the principles of practical reason, are the general premises of what Mill calls the "Art of Life" (1974, pp. 949-951). Together, the principles effectively restrict the domain of utilitarianism to "morally admissible" or "ideal" n-tuples of individual utility functions.[2]

For present purposes, it is convenient to summarize the seven principles of practical reason which can be identified in Mill's writings. Ignoring the bare statement of the principle of utility itself (Mill, 1969b, pp. 234, 237), which merely affirms that happiness or pleasure is ultimately the sole end and test of all human conduct (even though conduct can become disassociated from its original pleasurable antecedents through habit), the six remaining principles can be lexically ranked to reflect the doctrine of the higher pleasures. A very brief description of each axiom will be given since a detailed discussion of them has been provided elsewhere (Riley, 1985b, chap. 6-7):

1. **Principle of Social and Distributive Justice** (Mill, 1969b, pp. 257-258). Above all, any person i must make just choices whose consequences promote his (and everyone else's) security or self-protection. Security is the most desirable component of true happiness.

2. **Principle of Liberty** (Mill, 1977c, p. 223). As part of justice, any person i must generally refrain from interfering with any other person j's liberty of making self-regarding or private choices. Respect for liberty is generally the most desirable component of the vital interest in security (though justice authorizes rightful interferences with liberty in extraordinary circumstances).

3. **Principle of Individuality** (Mill, 1977c, pp. 266-267). As long as he meets his public obligations of justice, any person i should be free to make whatever private choices he wishes. Liberty of private choices is the only permanent source of his own self-improvement. Self-improvement (associated with aesthetic utility) is the second-most desirable component (after security) of true happiness.

4. **Principle of Charity** (Mill, 1977a, p. 11). Any person i should, at his own discretion, make charitable choices whose consequences at least maintain the existing level of goodwill in society.

Principles 1, 2, and 4 are principles of morality. They are properly parts of the general principle of right (and wrong):

5. **Principle of Right** (Mill, 1969b, pp. 210, 218). Any person i should make morally right public choices whose consequences promote social unity. Social unity is a permanent source ('possibility') of pleasure for him, or what is the same thing, is one of his permanent interests (the other being self-improvement). The two components of social unity are security (the concern of justice) and goodwill (the concern of charity).

6. **Principle of Expediency** (Mill, 1969a, p. 180). Finally, any person i should make the expedient public and private choices whose consequences maximize true happiness (in which his own happiness and the general happiness are harmonized).

Strictly speaking, it is these principles of practical reason (framed on the psychological theorems of the doctrine of the higher pleasures) which constitute the qualitative judgments used by the liberal utilitarian GSWFL to compare different kinds of utility.

Formally, the role of the principles of practical reason can be represented in terms of a suitable lexicographical ranking L over the k distinct kinds of utility:

$$\forall x\colon L = U_A(X) \infty U_B(x) \infty \ldots \infty U_K(x), \qquad (3)$$

where ∞ is a qualitative operator such that $\forall x\colon U_z(x) \infty U_z{}^1(x) \leftrightarrow U_z(x)$ dominates $U_z{}^1(x)$ irrespective of the intensity values at $U_z(x)$ and $U_z{}^1(x)$; $z, z^1 = A, B, \ldots, K$; $z \neq z^1$.

In particular, U_A refers to utilities of the kind denoted justice, U_B to utilities of the kind denoted privacy, U_C to charity. This lexical ranking reflects the qualitative judgments made by the experienced members of society. In effect, an ideal liberal way of life or morality (as seen by the experienced members of society) is being used to restrict the domain of the collective choice rule.

Turning now to the second component of the liberal utilitarian GSWFL, that is, the aggregation procedures for utility values of the same kind, Mill again goes beyond the usual utilitarian view which simply presupposes that homogeneous utilities are cardinally comparable in terms of intensity. As Sen suggests, Mill sometimes denies ethical status to intensity-aggregates even in this homogeneous utility context (Sen, 1981, pp. 191-194). In particular, he prescribes rights to liberty with respect to the so-called self-regarding or private kind of utility (Mill, 1977c, pp. 223-224). On my interpretation, these libertarian rights are general claims on society to protect the relevant individual from interference by others within his private sphere. Such rights are absolute for experienced individuals but are rightfully abrogated for inexperienced persons in rather extraordinary circumstances where justice demands interference, for example, if an agent would unintentionally cause himself grave physical harm through his rights-exercising (Mill, 1977c, pp. 291-304. See also Berlin, 1969, pp. lx-lxi).[3] In any case, libertarian rights clearly involve protection of the rightholder from the effects of interpersonal comparisons of intensity with respect to his private kind of utility. I will discuss this point further below.

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[2] Although Mill views the normative principles as a posteriori values inferred from the experience of reasoning and imaginative persons, it should be noted that the principles could also be viewed as a priori axioms given independently of experience à la Kant.

Even with respect to nonprivate kinds of utility values, however, Mill seems to reject the desirability of basing quantitative judgments on sums of merely subjective preference intensities. In this regard, it is important to recognize that Mill supposes quantities of pleasure (as much as qualities) to be facts of human nature, that is, facts whose experience is invariant with respect to personal identity. For him, quantities of pleasure are phenomena that any human mind discovers are uniformly associated with various circumstances (in accord with universal laws of causation). The mind does not properly create these quantities itself in some arbitrary fashion, independently of our experience of the actual world. By implication, given that Mill affirms a cognitive rather than a noncognitive utility concept, the true or objective quantity of utility experienced by any person in given circumstances must be universalizable in some sense, although mere universalizability is of course not sufficient for cognitive utility.[4] (For recent discussions of cognitive utility concepts in the context of uncertainty, see Eells, 1982; Hammond, 1983; Harsanyi, 1983; and references cited therein.)

On Mill's approach, therefore, experienced rather than inexperienced persons also have a key role to play in making quantitative judgments with respect to any given kind of utility. As Mill says, "What means are there of determining which is the acutest of two pains [of the same kind], or the intensest of two pleasurable sensations [of the same kind], except the general suffrage of those who are familiar (i.e., have experienced) both?" (1969b, p. 213). In particular, it is experienced rather than inexperienced persons who should make the interpersonal comparisons required to aggregate any nonprivate kind of utility. To make such comparisons, any experienced person can be assumed to place himself imaginatively in the positions of all other persons in society, judge their true personal utilities of the relevant kind when in each of their positions, and form an extended preference ordering over the Cartesian product of the sets of persons and states ($X \cdot N$) in order to compare the various personal utilities including his own. (For discussion of this extended sympathy interpretation of comparability in different contexts, see Sen, 1970, pp. 131-160; 1977, 1979b; and Suzumura, 1983.) The Experienced person's extended preference ordering is then used to generate a social preference relation for the given kind of utility under consideration. The social preference relation in question can be described as that which is most reasonable for the given kind of utility from the viewpoint of experienced persons. If there is more than one experienced person, then they are assumed to infer the same extended preference ordering (without necessarily agreeing over their own personal utility rankings). Ideally, if all persons in society are experienced, then they are assumed to agree on a single extended preference ordering under Sen's (1970, p. 156) axiom of complete identity (which does not imply unanimity over personal utility rankings).

It should be noted that the class of persons termed "experienced" generally varies across the different kinds of utility. In a given society such as our own, the vast majority of adults may be assumed to be experienced with respect to the lower kinds of utilities and disutilities, while at the same time only a distinct minority may have experienced the higher moral and aesthetic kinds of pleasures. As a result, the number of persons who are assumed to agree on an extended preference ordering will generally vary with the kind of nonprivate utility being considered.

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[3] When stating his famous principle of liberty, Mill (1977c, pp. 223-224) prescribes absolute libertarian rights for self-regarding actions, that is, *intentional* private conduct. He apparently assumes that no person can intentionally do himself an injustice. Any person could only *unintentionally* cause himself such grave injury, while emotionally disturbed or the like. Later in *On Liberty* (pp. 292-301), Mill prescribes interference with self-regarding behavior which would cause (with certainty) severe injury (e.g., death, servitude) to the agent. Apparently, such behavior must by its very nature be unintentional. He also prescribes interference with "seemingly" self-regarding conduct which is really other-regarding conduct involving harm to others; for example, he prescribes interference with a person who desires to drink alcohol when that person is known from experience to become violent toward others when drunk. Experienced persons apparently do not engage in unintentional conduct; that is, they fully anticipate the (probable) consequences of their behavior. For further discussion, see Riley (1985c).

[4] For discussions of various formalizations of the universalizability criterion (usually traced to Kant), see Sen (1970, pp. 131-160) and Rabinowicz (1979). For Mill's rejection of Kant's formulation of the criterion, see Mill (1969b, pp. 207, 249).

The prescription that experienced persons alone should make interpersonal comparisons for any nonprivate kind of utility precludes the sorts of ethical problems that arise when an inexperienced person's subjective intensity of pleasure is taken to be a reliable indicator of the true quantity of happiness he is experiencing under given circumstances. For example, it seems patently unfair from the point of view of any liberal norm of "equal treatment and respect" (cf. Dworkin, 1978b), that any two persons *i* and *j* who find themselves in the same circumstances and experience the same objective quantity of pleasure at a given state *x*, should be treated differently by the collective choice rule. But this is precisely what does happen in the usual utilitarian framework if the inexperienced person *i* mistakenly judges that his feeling of pleasure is twice as intense as person *j*'s, and person *i*'s subjective judgments are relied upon when determining the social choice. Indeed, on the usual utilitarian view, a fanatic who is under the illusion that his feelings are infinitely more intense than any other person's can invariably receive infinitely more weight than anyone else in the utilitarian calculus; that is, only the fanatic's subjective feelings of pleasure will count for any given kind of utility. To remove this sort of problem, Mill (at least implicitly) confines to experienced persons the making of quantitative judgments with respect to any two persons' utility values of the same nonprivate kind. As a result, the ignorant or malicious quantities of utility that are mistakenly reported by inexperienced persons will have no weight in the utilitarian aggregation procedure for any given nonprivate kind of utility (although such persons will nevertheless be granted libertarian rights with respect to the *private* kind of utility). Instead, the utilitarian procedure relies on experienced persons to infer correctly the true quantities of utility that any person *i* experiences in his position at any state *x*, and also relies on them to form an appropriate extended preference ordering to reflect these objective quantities. Thus, by virtue of this implicit domain restriction, a Millian utilitarian can quite agree that mere subjective intensity of pleasure is a highly dubious indicator of happiness (i.e., what is desirable) unless (as is notoriously not the case), all persons correctly judge and infer what the true quantities of pleasure actually are in various circumstances.

Formally, the liberal utilitarian aggregation procedures for utility values of the same kind can be represented as follows. For the *k*-1 public kinds of utilities, there is a series of *k*-1 utilitarian social decision functionals of the usual form:

$$\forall z\colon R_z = f_z([U_{z(i)}]), \tag{4}$$

such that

$$\forall x,y\colon xR_zy \leftrightarrow \sum_{i=1}^{n} U_{z(i)}(x) \geqslant \sum_{i=1}^{n} U_{z(i)}(y),$$

where $U_{z(i)}$ denotes person *i*'s utility value of kind *z*, $z=A, C, \ldots, K$; the omitted kind of utility being the *private* kind U_B.

According to equation (4), for any given nonprivate kind of utility, society (weakly) prefers *x* to *y* if and only if the aggregate quantity of that kind of utility at *x* is at least as large as the aggregate quantity of that kind of utility at *y* (the respective quantities being judged by experienced persons). These *k*-1 utilitarian social decision functionals yield *k*-1 complete and acyclic social preference relations R_z, one for each of the *k*-1 public kinds of utilities.[5] For the remaining private kind of utility, libertarian rights prevail:

$$R_B = f_B([U_{B(i)}]), \tag{5}$$

such that for any pair (x,y) where *x* differs from *y* only in respect of some feature of person *j*'s station:

$$xR_By \leftrightarrow U_{B(i)}(x) \geqslant U_{B(i)}(y);$$

and such that for any pair (w,z) where *w* differs from *z* only in respect of the *m* stations of the *m* members of a voluntary group *S:*

$$wR_Bz \leftrightarrow \forall_{i \in S}\colon U_{B(i)}(w) \geqslant U_{B(i)}(z).$$

According to equation (5), for any person *i*'s private kind of utility, society (weakly) prefers *x* to *y* if and only if person *i*'s private utility at *x* is at least as large as *i*'s private utility at *y* (the respective quantities being judged—however mistakenly—by person *i* himself). Similarly, for any voluntary group *S*'s private kind of utility, society

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[5] In general, each of the public R_z is acyclic but not transitive or even quasi-transitive. See Riley (1985a, chap. 5).

(weakly) prefers w to z if and only if group members' private utilities at w are unanimously at least as large as their private utilities at z (again, the respective quantities being judged by each group member himself). This libertarian aggregation procedure yields a complete social preference relation R_B for the private kind of utility, but R_B may be contaminated by cycles (Gibbard, 1974, pp. 388-397). The cyclicity of R_B is of no consequence for the generation of a final liberal utilitarian social ordering, however, because the complete and acyclic social preference R_A associated with utilities of justice is assumed to take lexical priority over R_B according to equation (3). In general, R_A will exhibit social indifference over those segments (possibly extensive) of R_B which exhibit cycles. (For further discussion of this point, see Riley, 1985a, chap. 5, 8-9.)

To sum up, the liberal or Millian utilitarian GSWFL as described is a mixture of two rather extraordinary components. In the first place, it reflects a liberal doctrine of the higher pleasures according to which certain kinds of utilities (in particular, justice and privacy) are incomparably more desirable than others for maximizing the general happiness. Secondly, when aggregating individual utilities of the same kind, the GSWFL extends equal and impartial treatment to different individuals, first, by granting each of them libertarian rights with respect to their private kind of utilities, and second, by relying on universalizable quantitative judgments which (so far as possible) ignore the illusions held by inexperienced persons (for example, children and fanatics) with respect to any public kind of utilities. In my view, this complex mixture of qualitative and quantitative components underlies Mill's much maligned attempt in *Utilitarianism* to clarify the proper connotation of the principle of utility:

> The maxims of equality and impartiality, . . . both in popular estimation and in that of the most enlightened, are included among the precepts of justice. . . . [These maxims are] involved in the very meaning of Utility, or the Greatest Happiness Principle. That principle is a mere form of words without rational significance, unless one person's happiness, *supposed equal in degree (with the proper allowance made for kind),* is counted for exactly as much as another's. *These conditions being supplied,* Bentham's dictum, 'everybody to count for one, nobody for more than one', might be written under the principle of utility as an explanatory commentary. (1969b, pp. 257-258; emphasis added)

It should be reemphasized that experienced persons play a crucial role in defining the domain restriction associated with liberal utilitarianism. These persons are assumed to classify and lexically rank the different kinds of utility; assign and respect libertarian rights with respect to private utility values; and make the interpersonal comparisons required to aggregate over any nonprivate kind of utility.

Formally, the liberal utilitarian GSWFL can be represented as the composition of the lexical ranking L given in equation (3) and the k distinct aggregation procedures (one for each of the k kinds of utility) given in equations (4) and (5). In this regard, equation (1) can be restated as follows:

$$R = L \circ \{f_A([U_{A(i)}]), f_B([U_{B(i)}]), \ldots, f_K([U_{K(i)}])\}, \tag{6}$$

where $f_z([U_{z(i)}]) = R_z(= U_z)$, $z = A, B, \ldots, K$; and $L = U_A \infty U_B \infty \ldots \infty U_K$.

According to equation (6), k distinct social preference relations R_z are generated by the relevant $f_z(\cdot)$, one for each of the k kinds of utility values; and then a meta-ranking over the R_z is induced by the lexical ranking L, permitting a final social ordering R to be determined. (For further discussion of the mechanics of the liberal utilitarian GSWFL, including concrete illustrations of the various sorts of responses that are made to Gibbard's rights-exercising paradox and Sen's Paretian liberal paradox, see Riley, 1985a, chap. 5-9.)

Different Levels of Utilitarianism

Mill suggests that any society that seeks to attain "utility in the largest sense" must first pass through various stages of development or improvement in which the general utility can be maximized in only a very imperfect sense (if at all) from a liberal perspective (Mill, 1969b, pp. 227-233; 1974, Book VI; 1977b; 1977c, pp. 260-275; 1977d, pp. 374-421; 1984a, b). By a stage of development, Mill apparently means the moral and intellectual habits generally practiced by the members of society. Corresponding to each such stage or level of social development (from the crudest to the ideal) is a particular level or version of utilitarianism. The various levels or versions of utilitarianism are all implicit within the liberal utilitarian GSWFL framework. The framework thus introduces a dynamic element by comprehending the possibility that any given society could move (up or down) among the various levels of utilitarianism.[6]

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[6] In my view, Mill's theory of social development is deserving of further study, but such a study is properly beyond the focus of the present article.

The remainder of the present essay concentrates on the ideal level of liberal utilitarianism. The ideal level corresponds to the highest or "most civilized" development of "moral beliefs and modes of thinking" that can be attained by society from a liberal perspective. At this highest stage, every person is perfectly "experienced." He has developed the ideal intellect required to identify invariably the true quantities and qualities of utility involved in given circumstances, and has cultivated the ideal character required to act invariably on the basis of his knowledge (Mill, 1979, p. 453). In effect, each person is the very epitome of "liberal education": he has acquired knowledge of human nature, or, as Mill would say, he has discovered a "true science" of psychology and ethology and uses that knowledge to maximize his true happiness. There is thus no need for one part of society to impose qualitative and quantitative judgments of utility on the remaining part of society. Instead, every citizen is willing and able to make these judgments himself. In this regard, any person i determines his ideal personal utility function W_i by correctly identifying his k kinds of actual utility functions $U_{z(i)}$, and lexically ranking the different functions in accord with the principles of practical reason:

$$\forall i: W_i = L \circ \{U_{A(i)}, U_{B(i)}, \ldots, U_{K(i)}\}. \qquad (7)$$

This ideal personal function W_i is a real-valued (as opposed to vector-valued) cardinal comparable representation of person i's true personal welfare and might thus be termed person i's welfare function (to distinguish it from his actual utility functions). Of course, every person i is assumed to arrive at the same ideal extended utility function (or extended preference ordering) under Sen's complete identity axiom. This ideal extended ordering provides the basis for aggregating the W_i.

In this ideal context, the liberal utilitarian GSWFL clearly reduces to an ideal utilitarian SWFL operating over a restricted domain of ideal utility n-tuples $[W_i(\cdot)]$. Representing the ideal utilitarian SWFL as $G(\cdot)$, we have:

$$R = G([W_i]) \qquad (8)$$

such that

$$\forall x,y: xRy \leftrightarrow \sum_{i=1}^{n} W_i(x) \geq \sum_{i=1}^{n} W_i(y),$$

where $W_i(x)$ is the cardinal (unit) comparable ideal utility value assigned by experienced person i to state x.

It should be noted that the real-valued aggregate utility function $W(\cdot)$ which represents R is apparently what Mill means by utility in the largest sense.

Despite the possibility of such an ideal utilitarian SWFL, societies whose members have not perfected liberal characters will fail to implement it. The ideal utilitarian process is ideal because its feasibility depends on all persons making the kinds of choices (both private and public) which really do maximize the quantity and quality of aggregate pleasure. Many different non-ideal levels or versions of the liberal utilitarian GSWFL framework can be distinguished, of course, each corresponding to some more or less inexperienced or illiberal "climate of moral and intellectual opinion" in the given society. For example, at a crude or base level corresponding to an uncivilized or barbarian stage of social development, every person i can be assumed to insist thoughtlessly that all utility is homogeneous. In that case, every person i rejects any notion of cognitive utility (including the idea that some kinds of utility are intrinsically more desirable than others), and is left with his crude subjective utility function $U_i(\cdot)$. In this case, the liberal utilitarian GSWFL collapses to the crude Benthamite utilitarian SWFL commonly attacked in the literatures of political theory, social choice theory, and moral philosophy. (For compelling attacks on this crude version of utilitarianism, see Sen, 1979a; Sen & Williams, 1982; and Williams, 1978, 1981; a survey of such attacks is given in Riley, 1985a, chap. 4.)

The multilevel feature of the liberal utilitarian framework carries interesting implications for the unrestricted domain condition.[7] Unrestricted domain can be satisfied within the framework. But if any logically conceivable n-tuple $[U_i]$ is admitted by the utilitarian GSWFL, then the given society is relegated to a particularly crude or low level of utilitarianism, for example, preference utilitarianism. To ascend to higher levels, society must restrict the domain of admissible utility n-tuples until finally, at the ideal or highest stage, only ideal utility n-tuples $[W_i]$ are admissible. The domain restriction in this context need not be ad hoc. It is as reasonable as the moral and aesthetic argument used to justify it. In this regard, as the revisionist interpreters of Mill have recently emphasized, objections to Mill's own argument in support of utility in the largest sense are not conclusive (see Berger, 1984; Gray, 1981, 1983; Riley, 1985a, b).

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[7] For other discussions of the "multi-level" aspect of Mill's utilitarianism, see Wollheim (1979) and Gray (1981). Hare's distinction between "two levels of thinking" in his version of utilitarianism also bears some similarities to the framework I attribute to Mill, although there are also important differences. See Hare (1976, 1981).

To facilitate our understanding of the ideal of utility in the largest sense, it is convenient to gain a little more insight into the distinction between private and public kinds of utility values. This distinction is fundamental for the possibility of liberal democracy and, in what follows, I will largely ignore the various other kinds of utility values.

Public and Private Spheres

Mill's vector concept of utility assumes that a positive distinction can be drawn between public (or other-regarding) and private (or self-regarding) kinds of utilities (Mill, 1977c). For example, it seems plausible to interpret private utilities as those kinds of desires or pleasures whose associated physical consequences *do not* harm or benefit any other person; and public utilities as distinct kinds of desires whose associated physical effects *do* harm or benefit at least some other person. Another interpretation sees private utilities as those kinds of utilities which *do not* necessarily alter the position or station of any person except the agent himself; and public utilities as distinct kinds of utilities which *do* necessarily alter the position or station of at least some other person. Nevertheless, despite these possible interpretations, any public-private distinction invites controversy, generally because of ambiguities or disagreements surrounding the meanings of such key terms as "harm," "benefit," and "station" (for recent discussions, see Berger, 1984; Gray, 1983; Ten, 1980). These ambiguities and disagreements are not likely to be completely eradicated in the near future, but it can be argued that credible working definitions are available for the key terms in question. In this regard, Mill himself can be interpreted at least implicitly to propose some credible definitions. For example, he seems to suggest that any person *i* is harmed by a person *j* (including *i* himself) only if *i*'s present capacity to determine his own choices (i.e., act as an autonomous agent) is impaired by *j*'s actions; person *i* is not harmed if he merely dislikes what *j* is doing. Similarly, any person *i* is benefitted by *j* only if his present capacity to act autonomously is improved by *j*'s actions. Moreover, if I want to say that person *i*'s public utilities necessarily alter the stations of other persons whereas *i*'s private utilities do not, then any person's station should be defined to include only those of his features and circumstances which have a bearing on his capacity to act as an autonomous agent. Only features the alteration of which would either impair or improve the person's capacity to make his own choices are included as components of his station, for example, bodily features, liberty, education, or material wealth. But a person's capacity to act autonomously is not affected by whatever his present whims or desires happen to be (although whether and to what extent he makes use of his capacity does depend on his present desires). Hence, a person's current subjective features are not components of his station on this reading of Mill. (For a more detailed discussion of Mill's key concepts, see Riley, 1985b, chap. 5-7; 1985c.)

Given a positive distinction between private and public utilities, I can define any person's private and public spheres accordingly. In person *i*'s private sphere of self-regarding utilities, his behavior does not impose harms or benefits on other persons. As Mill remarks, *i*'s private sphere comprises "all that portion of [his] life and conduct which affects only himself, or if it also affects others, only with their free, voluntary, and undeceived consent and participation" (1977c, pp. 225-226). Person *i*'s private conduct does not alter the stations of other persons unless they willingly agree. On the other hand, in person *i*'s public sphere of other-regarding utilities, his behavior does impose harms or benefits on other persons. To paraphrase Mill, *i*'s public sphere is composed of all that portion of his life and conduct which affects other persons whether or not they wish to be so affected. Person *i*'s public conduct necessarily alters the station of at least some other person *j* because *j* involuntarily participates in *i*'s conduct: *j*'s consent is forced by natural circumstances, or by person *i*'s deceit, or by *i*'s superior command over physical resources (including third parties).

Mill gives some concrete indication of private kinds of utilities in *On Liberty* (1977c, pp. 225-226). Person *i*'s private behavior apparently includes his internal "acts of mind" (whose physical effects cannot be felt by other persons) and those of his acts whose physical effects do not necessarily alter the present capacities of other persons to make their own choices. Of the latter acts, Mill specifically mentions expressing and publishing opinions, choosing personal lifestyles, and forming groups, always with the caveat that the opinions, lifestyles and groups in question must not be designed so as necessarily to alter the stations of other persons against their own wishes.

Formally, I define any person *i*'s distinct private and public spheres as follows: Consider the set X^2 of all feasible pairs of social states over which binary individual choices may be defined. A person *i*'s private sphere, denoted as $\bar{X}_i^2$, is a proper subset of X^2 and includes all pairs (x,y) where x differs from y only in respect of features of *i*'s own position or station (as defined above);

or, if other stations in addition to *i*'s are involved, where x differs from y only in respect of features of the m stations of the m members of a voluntary group S whose members $(i,j \ldots m)$ are not coerced, deceived, or in some way delirious. Person *i*'s self-regarding choices are defined over the pairs belonging to $\bar{X}_i^2$. I assume that person *i*'s choice over (x,y) must satisfy two conditions to be called private or self-regarding: first, x must differ from y either in respect of features of person *i*'s station alone, or in respect of features of stations of members of a group that *i* has voluntarily joined; and second, person *i* must be making the choice.

Person *i*'s public sphere, denoted as $(X^2 - \bar{X}_i^2)$, is also a proper subset of X^2 and includes all pairs (x,y) where x differs from y in respect of what might be termed real externalities which cause harm or benefit to some other person *j* by necessarily altering *j*'s station, whether or not *j* wishes. Person *i*'s other-regarding choices are defined over the pairs belonging to $(X^2 - \bar{X}_i^2)$. I assume that person *i*'s choice over (x,y) must satisfy two conditions to be called public or other-regarding: first, x must differ from y in respect of features of some other person *j*'s station (as defined above); and second, person *i* must be making the choice against *j*'s will. These two conditions together establish that real externalities are involved in person *i*'s other-regarding choices.

Given these definitions of individual private and public spheres, the private spheres of any two persons *i* and *j* will overlap at any given pair (x,y) if and only if x differs from y solely in respect of features of a voluntary group including those two persons. The private spheres of any two persons *i* and *j* are distinct, however, except for the pairs involving voluntary groups of which *i* and *j* are both members. At the same time, the portion of *i*'s private sphere which is distinct from *j*'s, will appear in *j*'s *public* sphere (because any choices *j* makes over these pairs would harm *i*, assuming that *i* merely wishes to be left alone in these private matters). A choice which any person *j* makes over any other person *i*'s self-regarding pair (x,y) is by definition an other-regarding choice, unless x differs from y in respect of the features of a voluntary group icluding *i* and *j*.

Clearly, any person *i* will generally have private and public spheres of choice which are different from those of any other person *j* in society. Each person's two respective spheres are unique. I will also refer to society's private and public spheres as distinct from those of any person. The importance of this distinction between the social, the public, and the private will, I hope, become more apparent in a moment. Society's private sphere, denoted as $\bar{X}^2$, is the mathematical union of all n persons' private spheres:

$$\bar{X}^2 = \bigcup_{i=1}^{n} \bar{X}_i^2, \tag{9}$$

where $\bigcup_{i=1}^{n}$ denotes the operation of union over n sets.

Society's private choices are defined over the pairs belonging to $\bar{X}^2$. In general, $\bar{X}^2 \subset X^2$ because there will be at least some pairs (x,y) such that x differs from y by real externalities which alter at least one person's station against his will. On the other hand, it is also logically conceivable (though empirically irrelevant) that $\bar{X}^2 = X^2$, in which case every feasible choice by society is also a self-regarding choice of some individual or group in society; or that $\bar{X}^2 = \phi$, in which case no feasible choice by society is also a self-regarding choice of some individual or group.

Society's public sphere, denoted as X^{*2}, is the union of all n person's public spheres:

$$X^{*2} = \bigcup_{i=1}^{n} (X^2 - \bar{X}_i^2). \tag{10}$$

Society's public choices are defined over the pairs belonging to X^{*2}. In general, $X^{*2} \subset X^2$, although it is initially quite plausible to think that $X^{*2} = X^2$. Except in a very special case, the plausibility arises because the pairs found in any person *i*'s private sphere $\bar{X}_i^2$ also appear as pairs in at least one other person *j*'s public sphere $(X^2 - \bar{X}_j^2)$. For example, the pairs in $\bar{X}_i^2$ whose elements differ only in respect of *i*'s station, also appear as pairs in the public spheres of each of the remaining n-1 persons other than *i*. Moreover, the pairs in $\bar{X}_i^2$ whose elements differ only in respect of the stations of an m-member voluntary group $S \subset N$ (i.e., $m<n$), also appear as pairs in the public spheres of the n-m persons outside S. Only those pairs in $\bar{X}_i^2$ whose elements differ in respect of the m stations of a society-wide voluntary group $S = N$, where every person in society is a member of the group, fail to reappear as pairs in some other person *j*'s public sphere.

It is by virtue of these latter quite special pairs (involving a voluntary group which is the size of society itself) that the private sphere $\bar{X}^2$ is generally not a subset of the public sphere X^{*2}, implying $X^{*2} \subset X^2$. On the other hand, it is also logically conceivable that $X^{*2} = X^2$, in which case no feasible choice by society is the private choice of a society-wide voluntary group; or that $X^{*2} = \phi$, in which case every feasible social choice is a private choice of such a societywide voluntary group.

In general, society's private sphere $\bar{X}^2$ and public sphere X^{*2} are distinct but overlapping sets of pairs. The two sets overlap at every pair whose elements differ only in respect of the station of some individual, or only with respect to the stations of the members of a voluntary group which is a proper subset of society. As a result, some of society's public choices are defined over the same pairs as some of society's private choices. I beg the reader's indulgence on this point temporarily.

I should mention again that in addition to private and public spheres, one can identify various other spheres of pairs of alternatives. For example, person i's private sphere $\bar{X}_i^2$ includes an aesthetic subsphere over whose pairs are defined his aesthetic choices (or his ugly choices, depending on the taste he displays). Similarly, within person i's public sphere $(X^2 - \bar{X}_i^2)$ is a subsphere of morality over whose pairs are defined his moral or immoral choices; and his department of morality is further divided into his complementary subspheres of charity and justice. Again, *society's* departments of aesthetics and morality (charity and justice) will consist of the unions of the respective individual spheres. A complete elaboration of the ideal utilitarian framework requires a notation to account for these various subspheres or departments of pairs of alternatives. To simplify my present discussion of the framework, however, I do not explicitly introduce these refinements here.

Finally, I reemphasize that the distinction between public and private spheres permits the introduction of a normative interpretation of interpersonal comparisons such that comparisons are generally rejected as undesirable within private spheres. This sort of normative interpretation is presumably of interest to liberals because the idea of liberty seems to imply that society should (at least generally) protect any person i from the consequences of comparability within his private sphere $\bar{X}_i^2$.

The moral rejection of interpersonal comparisons in private matters may help to assuage liberal critics of comparability such as Arrow (1963, 1973), Hayek (1960) and Riker (1982). Indeed, it is a plausible conjecture that their distaste for interpersonal comparisons is more a reflection of their implicit liberal values than it is a scientific assessment. Arrow's main fear, for example, seems to be that comparisons (if feasible) would be used in an illiberal fashion, to destroy personal autonomy:

> If your satisfaction depends on some inner qualities that I do not possess, then I really have not had the experience which will enable me to judge the satisfaction one would derive from that quality in association with some distribution of goods. . . . [Moreover], in a way that I cannot articulate well and am none too sure about defending, the autonomy of individuals, an element of mutual incommensurability among people seems denied by the possibility of interpersonal comparisons. No doubt it is some such feeling as this that has made me so reluctant to shift from pure ordinalism, despite my desire to seek a basis for a theory of justice. (1977, p. 225)

The implicit fear of an imposed social homogeneity apparently leads Arrow toward pure ordinalism and its severe informational restraints. (For a discussion of these informational restraints, see Sen, 1979a.) In other words, his liberal values may well be responsible for the attraction he feels toward factual (informational) premises that are inadequate to support a liberal theory of justice. If so, Arrow's attraction to pure ordinalism is misplaced.

With the characterization of public and private spheres now before us, it is possible to indicate how the ideal utilitarian SWFL brings about a rational harmony between the values of liberty and democracy. The formal nature of this rational harmony will be clarified by the axiomatization presented in the next section. As Bernard Williams has asserted in another context, "If philosophy is to understand the relations between conflict and rationalisation in the modern world, it should look towards an equilibrium—one to be achieved in practice—between private and public" (1981, p. 82).

Axiomatization of Ideal Liberal Utilitarianism

Without necessarily denying the possibility of alternative ethical foundations for liberal democracy, liberal utilitarianism can be defended as an attractive approach to democratic theory. In particular, the ideal utilitarian SWFL can be defended as an ideal "liberal democratic" collective choice rule. The following axiomatization demonstrates the logical conceivability of an ideal harmony of liberal democratic values.

To facilitate the axiomatization, it is convenient to offer idealist reinterpretations of some familiar social choice axioms. Specifically, I will interpret Condition P^* (strong Pareto principle), Condition *SN* (strong neutrality or welfarism), Condition *SA* (strong anonymity) and Condition L^* (liberty) such that they are defined in terms of ideal personal welfare functions $W_i(\cdot)$ as opposed to crude subjective utility functions $U_i(\cdot)$. Condition U (unrestricted domain) will be dropped; that is, the domain of the ideal utilitarian SWFL is restricted to ideal utility n-tuples $[W_i]$.

The ideal axioms will be distinguished from their more common counterparts by using the bracketed letter '*r*' to denote revised; for example, Condition *SN(r)* will denote the revised welfarist axiom. The revised axioms are ideal in the sense that they are defined in terms of individual welfare functions which prescribe what the individual should choose by describing what he would choose if he were an experienced person. The more common axioms, on the other hand, are defined in terms of individual utility functions which merely describe what the individual *does* choose (whether or not he is experienced).

The ideal axioms are stated as follows:

> *Condition SN(r).* For any two ideal *n*-tuples $[W_i]$, $[W_i^1]$, $\forall x,y,a,b$: $[(\forall i: W_i(x) = W_i^1(a)) \;\&\; (\forall i: W_i(y) = W_i^1(b))] \rightarrow G([W_i(x), W_i(y)]) = G([W_i^1(a), W_i^1(b)])$.

The revised strong neutrality or welfarism condition asserts that the ideal utility information regarding any pair of social states is the only information that society should require for ranking that pair.

> *Condition SA(r).* If for any two ideal *n*-tuples $[W_i]$, $[W_i^1]$, there is a permutation function $\phi(\cdot)$ defined over the set N of individuals such that $\forall i$: $[(\exists x: W(x)) = W^1_{\phi(i)}(x) \;\&\; (\mathrm{V}y \neq x: W_i(y) = W_i^1(y))]$, then $G([W_i]) = G([W_i^1])$.

The revised strong anonymity condition asserts that society's ordering should remain invariant when the ideal utility values for any particular state x are permuted among the persons in society without doing anything to the ideal utility values for other states.

> *Condition P*(r).* $\forall x,y$: $[(\forall i: W_i(x) \geqslant W_i(y)) \;\&\; (\exists i: W_i(x) > W_i(y))] \rightarrow xPy$.

The revised strong Pareto principle says that if all persons assign at least as much ideal utility value to x as to y, and if some person assigns more ideal utility value to x than to y, then society should strictly prefer x to y. Revised weak Pareto and Pareto indifference conditions can be defined accordingly.

Perhaps surprisingly, revised liberty conditions are implicit within the revised Paretian conditions, that is, Paretianism ideally comprehends liberty. To illustrate this point, it is convenient to partition any person i's $W_i(\cdot)$ into two main substructures: an ideal private component $\bar{W}_i(\cdot)$ defined over the pairs included in i's private sphere $\bar{X}_i^2$; and an ideal public component $W_i^*(\cdot)$ defined over the pairs included in i's public sphere $(X^2 - \bar{X}_i^2)$. These ideal components are merely a convenient way of analyzing W_i in accord with the fundamental dichotomy between private and public issues.

For present purposes, a key point to recognize is that by definition any experienced person i acts justly in his private affairs, that is, all experienced persons correctly infer when issues of justice are involved, and any such person follows the private course of action which he thinks is most just for himself (and where relevant, the members of his voluntary group).[8] Moreover, no experienced person is any better than another at inferring what justice requires. As such, justice can never demand interference with an experienced person i's right to liberty within his private sphere $\bar{X}_i^2$ (as it can demand interference with an inexperienced person's libertarian rights). Other experienced persons are no better judges of what justice requires than i himself is, and can do no better than to grant i absolute libertarian rights over the pairs in $\bar{X}_i^2$.

In this ideal context, it is quite easy to show that liberty and respect for liberty are subsumed in the revised Paretian conditions, suitably interpreted. In this regard, a revised version of Condition L^* is defined exclusively in terms of persons' $\bar{W}_i$; that is, liberty is seen as an exclusively private sphere condition:

> *Condition L*(r).* For each and every person i in society, there exists at least one distinct pair $(x,y) \in \bar{X}_i^2$ such that i is strongly decisive over (x,y); that is, $\bar{W}_i(x) > \bar{W}_i(y) \rightarrow xPy$, and $\bar{W}_i(y) > \bar{W}_i(x) \rightarrow yPx$.

The revised liberty condition says that society should permit each person to choose as he likes over at least one distinct pair (x,y) belonging to his own private sphere. At the same time, respect for liberty is defined exclusively in terms of persons' W^*_i. In particular, any experienced person j invariably respects any other experienced person i's absolute right to be strongly decisive over $(x,y) \in \bar{X}_i^2$, by assigning $W^*_j(x) \geqslant W^*_j(y)$ if and only if $\bar{W}_i(x) > \bar{W}_i(y)$; and by assigning $W^*_j(y) \geqslant W^*_j(x)$ if and only if $\bar{W}_i(y) > W_i(x)$. But then liberty and respect for liberty are subsumed in the revised strong Pareto principle, suitably interpreted. Any person i's liberty over a given pair (x,y) is ideally protected by Paretianism as follows:

> *Condition P*(r).* $\forall x,y$: $\{(\forall i: W_i(x) \geqslant W_i(y)) \;\&\; (\exists i: \bar{W}_i(x) > \bar{W}_i(y))\} \rightarrow xPy$.

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[8] In general, any experienced person's ideal private component $\bar{W}_i$ is assumed to be fully intentional (i.e., it incorporates full knowledge of the probable consequences of his private behavior), with the proviso that no person can intentionally cause himself an injustice. Moreover, $\bar{W}_i$ is assumed to satisfy a separability condition (which removes the possibility of collectively cyclical libertarian rights-exercising of the Gibbard type). For a discussion of the properties exhibited by $\bar{W}_i$, see Riley (1985a, chap. 7-9).

On this interpretation, the revised strong Pareto principle says that if all persons assign at least as much ideal (private or public) utility to x as to y, and if some person i assigns more ideal private utility to x than to y, then society should strictly prefer x to y (and thus protect i's liberty over the pair in question). Note that every person $j \neq i$ is assumed to assign $W^*_j(x) \geqslant W^*_j(y)$ in respect of i's liberty. Paretianism is now seen as a hybrid condition straddling public and private spheres of choice.

It should be emphasized that Condition $L^*(r)$ is really an unacceptably weak condition from a liberal perspective: it captures only a small part of what we usually understand by the term 'liberty.'[9] The ideal utilitarian SWFL is able to accommodate much stronger liberty conditions. For example, the SWFL can accommodate the following "strong liberty" condition which may well satisfy even the most ardent libertarians:

> *Condition SL(r).* For each and every person i, for every distinct pair $(x,y) \epsilon \bar{X}_i^2$, i is assigned absolute libertarian rights over (x,y) such that:
>
> (a) if x differs from y only in respect of features of i's station, then $\bar{W}_i(x) > \bar{W}_i(y) \rightarrow xPy$, and $\bar{W}_i(y) > \bar{W}_i(x) \rightarrow yPx$; or
>
> (b) if x differs from y only in respect of features of the m stations of the m members of a voluntary group $S \subseteq N$ which includes i, then $\forall i \epsilon S{:}\ \bar{W}_i(x) > \bar{W}_i(y) \rightarrow xPy$, and $\forall i \epsilon S{:}\ \bar{W}_i(y) > \bar{W}_i(x) \rightarrow yPx$.

It is possible to strengthen further part (b) of Condition *SL(r)* by permitting S to determine xPy if all members weakly prefer x to y and at least one member strictly prefers x to y. But I will not pursue such possibilities here.

Given the ideal axioms, the ideal utilitarian SWFL is easily axiomatized. The axiomatization can be interpreted as demonstrating the possibility of an ideal liberal democracy. More specifically, the ideal utilitarian SWFL can be interpreted to consist of private and public components. The private component is absolute liberty of self-regarding $\bar{W}_i$. The public component is a strongly anonymous, strongly neutral, Pareto-inclusive public choice rule which aggregates over other-regarding W^*_i. The two components are consistently integrated within the ideal utilitarian SWFL.

The axiomatizations of the crude Benthamite utilitarian SWFL already presented in the social choice literature can be suitably reinterpreted to apply to the ideal utilitarian SWFL. For example, the axiomatization provided by d'Aspremont and Gevers (1977) serves to characterize the ideal utilitarian SWFL if the naive utility concept $U_i(\cdot)$ is replaced by the ideal utility concept $W_i(\cdot)$ in their proof. I will simply state the following theorem since its proof is such a straightforward adaptation of d'Aspremont and Gever's proof:

> **Theorem 1 (Ideal Utilitarianism).** A social welfare functional $G(\cdot)$ which satisfies Conditions *P*(r), SN(r), SA(r)* and cardinal unit comparability must be ideal utilitarianism.

Condition U (unrestricted domain) is, of course, dropped.

It is convenient to amplify Theorem 1 to illustrate the private and public components of the ideal utilitarian SWFL. The amplified version of the theorem demonstrates more clearly the possibility of an ideal liberal-democratic collective choice process:

> **Theorem 2 (Ideal Liberal Democracy.** If for any given feasible set X of social states, each person in society is sufficiently committed to liberal morality, that is, sufficiently experienced to identify correctly and rank lexically his different kinds of cardinal utilities at any state x, respect the absolute libertarian rights of other persons in their private affairs, and infer reasonable interpersonal comparisons with respect to public matters outside every person's private sphere, then an ideal liberal-democratic SWFL $G(\cdot)$ is possible such that:
>
> (a) for the ideal public utilities $W_i^*(\cdot)$ defined over pairs belonging to society's public sphere $X^{*2} \subset X^2$, $G(\cdot)$ satisfies Conditions *P*(r), SN(r), SA(r)* and (at least) cardinal unit comparability to yield a transitive (but generally incomplete) ideal "public preference ranking" R^* over X;
>
> (b) for the ideal private utilities $\bar{W}_i(\cdot)$ defined over pairs belonging to society's private sphere $\bar{X}^2 \subset X^2$, $G(\cdot)$ satisfies Condition *SL(r)* to yield a transitive (but generally incomplete) ideal "private preference ranking" $\bar{R}$ over X;
>
> (c) to harmonize ideal public and private kinds of choices and integrate R^* with $\bar{R}$, $G(\cdot)$ satisfies Condition *P*(r)* to yield a complete and transitive "social preference ordering" R over X.

Part (b) can be termed the "liberal" part of the collective choice process and part (a) the "democratic" part. The "liberal" and "democratic" parts are then harmonized in part (c).

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[9] It is generally recognized within social choice theory that Sen's liberty conditions are unacceptably weak from a liberal perspective. See Sen (1970, pp. 82-83; 1983, pp. 17-18).

I should note that the above axiomatization does not establish that the social choices produced by the ideal utilitarian SWFL are credible or reasonable. As Mill argues, there can be no formal proof that utility is the sole thing desirable for its own sake, or that utility in the largest sense is a credible concept of utility (1969b, pp. 234-239). Ultimately, the only possible proof of the credibility of ideal utilitarian collective choices is that all more or less experienced persons (or at least a majority of them) actually do desire to make such choices.

In summary, in an ideal liberal democracy, experienced persons will adopt distinct public and private perspectives on choice. From his public perspective, any such person feels more or less obligated to accept the different kinds of public choices generated for society by the strongly anonymous, strongly neutral, Pareto-inclusive public choice rule. At the same time, from his private perspective, he enjoys absolute libertarian rights to make any private choices he desires, although in extraordinary circumstances he may voluntarily interfere with his actual private inclinations to behave toward himself as he thinks justice demands.

Conclusion

As demonstrated, it is logically possible to conceive of a truly happy life in which absolute libertarian rights over private matters are consistently integrated with a democratic public choice rule in which all citizens have an equal voice over public matters. Indeed, this rational harmony of liberal democratic values can be taken to be the ultimate end which (however dimly perceived) in part motivates most citizens of current western societies and thus also forms a part of our actual experience. In this regard, recent critics and defenders of liberalism have both been far too hasty in dismissing the notion of an ideal liberal way of life. The sort of liberal democracy that survives in the absence of any such notion is necessarily sterile and impoverished, not worthy of serious defense.

Evidently much more needs to be said before liberal utilitarianism can be accepted as a credible (rather than merely consistent) ethical basis for democratic society. In concluding, I wish to touch on three general areas that require further clarification. (A more complete discussion of these topics is provided in Riley, 1985a, b.)

First, granted that the liberal utilitarian GSWFL provides a formal representation of the values and norms that must generally be found in any society if individual preferences are to be transformed into liberal democratic social choices, the question remains, how do these values actually manifest themselves in any given concrete circumstances? For example, which actions are actually inferred by experienced persons to be associated with the worst kind of pains, that is, injustices? Under which circumstances does any person's conduct actually constitute private behavior? The concrete details of the liberal democratic good life (including the descriptive theories of justice, privacy, and charity, among others) remain to be filled in. In this regard, it remains an open question how far the liberal utilitarian approach diverges in any practical sense from, for example, Rawls's neo-Kantian approach to liberalism. Despite their fundamentally different epistemologies, for example, both approaches assign lexical priority to a theory of justice which (whatever else it does) not only tolerates but encourages a wide variety of private lifestyles.

Second, given that rich utility information (even if technically feasible) is not always economically feasible, the question arises of how liberal utilitarianism (particularly the public choice component) may be institutionalized in the context of "poor" (i.e., ordinal noncomparable) utility information. The problem of designing utilitarian political institutions preoccupied Mill (1977d) and has recently been reemphasized by Sen and Williams (1982, pp. 1-21). In this regard, the design of public choice rules in a purely ordinal context has largely been the concern of formal voting theorists following in the tradition of Arrow. (For example, see Plott, 1976; Riker, 1982; Schofield, 1984; Schwartz, 1985.) Without going into details for present purposes, there are interesting formal links between liberal utilitarian values and majoritarian legislative institutions (combined with an appropriate system of proportional representation, where representation is generally expedient).

Finally, related to the problem of designing liberal utilitarian political and legal institutions, the general question arises of how any given society might be expected to develop towards "utility in the largest sense." In this regard, besides reinforcing liberal utilitarian values in the minds of the existing generation of citizens through suitably designed institutions, the society must of course transmit these values to new generations through a mandatory national system of liberal education. But ultimately, the key engine of liberal social development as emphasized by Mill (1977b, c, d; 1984a, b) is individual liberty. Indeed, perhaps the key characteristic of liberalism is this optimistic yet contentious claim that individuals and groups, left free to pursue their private ideas without interference from others, will tend to acquire the competence required to maximize the general happiness in terms of quality as well as quantity. A careful examination of this claim must await some other occasion.

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The New American Political Party

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To understand changes taking place within political parties we must work from a realistic theory, one that accepts these parties as office-seeking coalitions. On that premise I lay out three interacting sets of variables: 1) The structure of political opportunities, or the rules for office seeking and the ways they are treated, and 2) the party system, or the competitive relations among parties, define the expectations of politicians, and thus lead them to create 3) party organizations, or the collective efforts to gain and retain office. Hypotheses derived from the relations among these variables allow us to examine changes in American parties in the twentieth century. They explain why the Progressive era reforms, in tandem with the post-1896 party system, produced an uneven distribution of party organization and weak linkages among candidates and officeholders. The same theory also explains why changes taking place since the 1950s are producing greater organizational effort and stronger partisan links among candidates and officeholders.

America's political parties are alive and well. Despite more than a decade's forebodings of decomposition, decline, and disappearance, the Republicans and Democrats continue to win all the elections and form all the governments. In such vital signs as the numbers and distribution of their candidates, in the amount of organized effort put into recruiting and campaigning for them, and in the degree to which they cooperate with their fellow partisans in government, the two parties show signs of strength as great, if not greater, than they have at any time in the past 50 years. It should be clear by now that the grab bag of assumptions, inferences, and half-truths that have fed the decline-of-party thesis is simply wrong.[1]

In this article my object is less that of attacking the decline-of-party thesis than it is to try to understand what has in fact been happening to the parties. For the decline-of-party thesis is correct in at least one sense: the parties are different from what they were 20 to 30 years ago. To understand how and why they have changed, however, we must approach them with a realistic theory of parties, one that accepts them for what they are rather than imposes on them impossible norms such as those implicit in the responsible-party model, or theories derived from the study of aspects of politics peripheral to parties such as public opinion or voting behavior. I therefore start by outlining briefly the theory of parties that I believe to be implicit in most descriptive writings on parties.[2] Then I apply the theory to the broad development of parties in the twentieth century, distinguishing between the period leading up to the 1950s, which I call the "old" party and that which has been emerging since the 1950s, or the "new" party.

Elements of the Theory of Democratic Political Parties

A theory of parties requires a clear understanding of the key terms, "party," and "party organization." In American politics such an understanding has been confounded by our penchant for trying to control parties by laws, and the

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[1] The decline-of-party thesis pervades almost all contemporary writing. The established view is well stated by Gary Orren (1982, p. 31): "In a world in which political scientists disagree on almost everything, there is remarkable agreement among the political science profession that the strength of American political parties has declined significantly over the past several decades. Regardless of how one measures partisanship—by personal party identification within the electorate, by party discipline in Congress, or by the vitality of party machinery—there is massive evidence attesting to the weakened condition of the parties in the United States."

[2] By descriptive writings on parties I refer to the ways competent analysts explain why politicians behave the way they do and how they go about organizing support. The various writings, for example, of such perceptive students of parties as E. E. Schattschneider and V. O. Key, Jr., could readily be used to derive this theory.

[3] The theory is more fully laid out in Schlesinger (1965). There are few efforts to fit parties into any kind of general organizational literature. For a discussion of the problem in a comparative framework see Janda (1983).

efforts by states to lay out their structures and prescribe their procedures. In any organization some discrepancy will exist between the formal and the informal, or between the ways things are supposed to be done and the reality. The legal approach, however, serves only to magnify the gap between the real and the prescribed because, whatever else they may be, parties are certainly efforts to capture the offices of the state by the rules laid down by the state. Parties emerge naturally around those offices and rules as long as organization enjoys advantages over disorganization. Thus the legal definition of parties simply creates additional procedures which those who want office must organize to capture. Thus organization is not eliminated or reduced in importance; it does, however, become less visible, not because it has gone underground, but because it lies outside the boundaries of the prescribed party structure. The party and its organization to which this theory is addressed is the real or operative party rather than the formal or prescribed party.[3]

Definition

A political party is a group organized to gain control of government in the name of the group by winning election to public office. Note two things about this definition. First, it focuses on officeseekers. Thus by implication it excludes from the party all those who see themselves primarily as choosers among parties, that is, voters. The theory thus avoids all of the ambiguities that arise when we include voters in parties. Second, the definition implies that office seeking itself has a dominant place in the direction of democratic parties. Why that should be so given the complexities in goals in anything as varied and large as a political party is a distinct issue with which I have dealt elsewhere (Schlesinger, 1984). In this article I simply assume its dominance.

Party Organization

By organization I mean all of the cooperative, deliberate activities among two or more people aimed at capturing elective office in the name of the party. The organization of party theory therefore rejects the distinction between formal and informal, or prescribed and nonprescribed structures and procedures. Although at first this may appear to produce a very fuzzy and unfocused picture, in fact it is given a very sharp focus by the clarity of the party goal, capture of elective office. Whatever complexity we find in party organization comes from the variety of offices to be gained and the assorted rules and strategies that must be employed rather than from the multitude of perspectives or images people may bring to the parties. Note also that this view of organization does not slight the formal or legal party apparatus. To the extent that it does play a part in the attainment of elective office, it is very much to be considered within the party.

The basic unit of the party is the nucleus, which consists of the collective efforts to capture a single office. Many such nuclei can emerge within a single party even around one office, as many ambitious politicians create organizations aimed at capturing the party's nomination. The act of nomination, however, is the party's effort to reduce all these efforts to a single nucleus. Indeed, much of the history of the development of party organizations represents efforts to assure that they have but one nucleus or candidate for each office in the general election and also that they will have support from all elements of the party for that candidate.

The multinuclear party organization consists of all efforts to link nuclei together. These efforts may consist of deliberate acts such as the transfer of money or endorsements, and they may occur automatically if the efforts to win one office have an impact on the chances of the party's candidates for others, as in the coattail effect.

The party organization goes through a predictable cycle of three phases: nominations, elections, and government. The organizational tasks vary, but we assume that the goal of gaining and retaining office dominates each phase. The extent of linkages among nuclei will also vary with the phase. Thus we would expect ties during the governmental phase to be the greatest simply because government demands cooperation, and the ways in which a party cooperates will affect the ability of its candidates to gain reelection. On the other hand, we would expect somewhat less linkage during the nominating phase because it is then that all of the conflicting ambitions of the party's leaders are being sorted out. The linkages in any of the phases, however, are not a fixed or stable quantity, but vary with conditions. Indeed it is precisely the existence and character of such linkages that is at issue in the thesis that the parties are declining or in a state of decomposition. The central problem then for the theory of party is explaining why and where party nuclei will emerge and how they will link up with others to create multinuclear parties.

The Framework within Which Party Organization Develops

The democratic political party is a creature of its environment (Harmel & Janda, 1982). The most important aspects of that environment are those that shape the expectations and ambitions of the most important people in parties, their officeseekers. We can discern two categories of factors that direct their expectations.

The first is what I have called the *structure of political opportunities,* or the offices, the rules for attaining them, and general patterns of behavior surrounding their attainment (Schlesinger, 1966). The general character of this structure is of course laid out in the various constitutions and electoral laws. In the United States, from the very formation of the republic it has been a system designed to frustrate the type of combination implicit in political parties. The very fact that political parties did develop early in the United States is testimony to the naturalness of their formation once free elections are established. But their actual organization depended on the emergence of the second major factor, a party system.

By the *party system* I mean the relationship among parties, or particularly the relative chances that each party has of winning the various elective offices. In effect the party system may be thought of as each party's chances for office laid out in the structure of opportunities. Thus we know that the American system is a two-party system not because only two parties ever compete, but because only two parties have a chance at winning any of the significant offices. The central variable in the party system is the level of competitiveness for each office, something that differs greatly among offices and also over time.

Organization as the Interaction between Party System and Opportunities

By themselves the rules of office seeking tell us little about party organization. Whatever impact they have must be viewed along with that of the party system, which can be seen in the way that the development of a competitive system in the United States completely reshaped expectations about how some of the basic election procedures of the Constitution would work. For example, the framers of the Constitution thought they were turning the selection of the president over to a group of independent presidential electors. To assure that independence they adopted the same kind of device they used, successfully, to assure separation of powers: the separation of personnel, or the requirement that no member of the federal establishment could be a member of the College. By 1800, however, competition for the presidency had turned members of the College into one of the most highly disciplined groups of partisans to be found anywhere in American politics. That was organization! Similarly, the provision for selection of federal senators by state legislatures appeared to be a means for assuring state control of the federal senate. The growth of a national party system, however, turned the process around, and in many states the legislatures in their selection and operations became arms of their federal senators (Riker, 1955). Party organization thus is a product of the interaction between rules of office seeking and the party system that grows up to define how parties compete for those offices.

Party Organization

I focus on three facets of organization, each of which provides basic propositions about party organization based on the assumption that each develops in response to the goal of attaining elective office.

1. The *distribution* of party nuclei reflects first of all the existence of office worth organizing to capture. The rules of election also define the extent to which separate nuclei for each office can develop. Thus no separate party nucleus can emerge around the vice presidency because its election is tied to that of the president. Most offices in the United States, however, are elected independently, making separate party nuclei possible. The actual distribution of such nuclei rests upon the party system. From this I infer the following proposition:

> Party nuclei will emerge in those constituencies where the party has some short- or long-run chance of winning the office.

2. The *level of organizational effort* by each nucleus is also defined by its competitive status. I conclude therefore:

> In competitive constituencies, both parties will put forth a high level of effort.
>
> In one-party dominant constituencies both parties will put forth less effort than in competitive constituencies, but the dominant party will exert itself more than will the minority party.
>
> In purely one-party constituencies the effort of the controlling party will be minimal, that of the hopeless party will at best be a token effort.

3. *Multinuclear transfers* of contributions will follow along the lines laid out in the structure of political opportunities and also will reflect the needs of the various nuclei as they compete for office. I infer therefore:

> Nuclei in competitive constituencies need the most assistance and will be most likely to cooperate with other nuclei of the same party, to

> seek and give assistance in all phases of the organization, in nominations, elections, and in government.
>
> Nuclei in safe constituencies need little or no help and are thus least likely to cooperate with other nuclei in all phases of the organization.
>
> The more widespread are competitive constituencies, the greater the level of linkage among the various party nuclei.

I should note here that the theory of party as I have outlined it focuses narrowly upon three key factors: the structure of political opportunities, the party system, and party organization. Changes in any one of these will affect the others, and such changes lie within the scope of the theory. Thus, for example, any increase in organizational effort will tend to increase also the competitive level in the party system, or we can see how an expansion of the electorate can also raise the level of competition and thus affect organization. What the theory does not seek to explain are broad changes in the environment of the major variables themselves. Thus we know that the party system has been affected over time by the emergence of new issues as well as demographic changes. Party organization is affected by changes in the technology of organization, including new modes of transportation and communications. These are all external to the theory. My concern is with how the party organization is affected by changes in the major variables, whatever may have been the causes for those changes in the first place.

The Changing Context of American Party Organization in the Twentieth Century

With the elements of the theory of party in mind we are now in a position to see how American parties have changed. Neither of the two contextual variables, the structure of political opportunities nor the party system, have been static during this era. Yet I can say that in the twentieth century they have gone through two distinct periods: the first follows upon the Progressive era at the turn of the century, when major changes were made in the rules of election, and was accompanied after 1896 by a major restructuring of the party system. Together they produced the kind of weak, disjointed party characteristic of the period leading into the 1950s. These, of course were the parties at which the American Political Science Association's Committee addressed its major report, *Towards a More Responsible Two-Party System* (1950). Ironically, at about that time we can see the beginnings of changes taking place in the two major variables that would transform the parties, if not into exemplars of the responsible party model, at least into closer approximations of it.

The New Structure of Political Opportunities after 1900

The rules growing out of the Progressive era had the clear aim of weakening party organizations. The direct primary with a single blow deprived the existing party apparatuses of their most important power, the making of the party nomination for elective office. Ballot reforms deprived parties of many of the mechanisms they had developed for controlling the vote. Reforms within the Congress reduced the capacities of party leaders to direct the course of legislation.

None of these reforms, however, eliminated party organizations for the reason that none made organization any less useful to politicians. To the contrary, having to win two elections in order to gain office made it all the more important that candidates organize. The parties, however, would certainly be different because the structures they sought to capture were now different.

One critical difference was that the new rules reinforced the dominance of their parties by candidates and in particular by incumbent officeholders. The Australian ballot had eliminated the possibility that a candidate might be knifed by unfriendly partisans who could keep his name off the ballot in selected precincts.[4] The direct primary altered the nominating constituency to one in which incumbent officeholders had all the advantages.[5] When we consider that they had to be adopted state by state, the rapidity with which such reforms were adopted is testimony either to the strong impact of reform ideals on politicians of the time, or to the fact those who had to pass them into law could tell a good thing when they saw it. To the advantages incumbents now had in gaining renomination and election I must add the impact of the post-1910 revolt against the Speaker in the House of Representatives. In place of the system in which party leaders enjoyed great influence over the legislative agenda through the control of committees and procedures, the reformed Congress became one in which the familiar seniority rules became the major device for the distribution of positions and power (Fenno, 1965). The new rules pointed to a party in which, both in and outside of government, there would be no leverage for those who might wish to control candidates or officeholders. These were to be pure officeholder-candidate parties subject to control primarily through the ballot box.

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[4] For example, in the election of 1888, Cleveland failed to carry his home state of New York by a little over 12,000 votes. Democratic Governor David Hill carried the state by over 17,000 votes. Charges were made that in selected New York precincts Cleveland's name had not been on the ballot distributed by Democrats more interested in furthering Hill's national ambitions. Had Cleveland carried New York, one of the two instances in which the Electoral College failed to choose the candidate with the popular majority would not have occurred (Fredman, 1968, pp. 28-29).

[5] A common practice in the nineteenth century was to rotate nominations among counties or towns in a district. Those who pressed for the primary were well aware that it would help incumbents. "In helping to break down the system of party nomination by convention in Massachusetts and replacing it with direct nomination, in 1902, one of the purposes I had in mind was to lessen the damage wrought by these rotation arrangements" (Luce, 1924, p. 365).

The second major effect of the new rules was that they severed many of the links of the past which had forced elements of the parties to work together. For example, the nineteenth century party ballot was not only the product of strong organization, it required strong organization if it were to work. Ballots had to be printed and distributed in every precinct. A party without some kind of local organization was a party that would lose some of its votes. Also, of course, the ballot encouraged straight party-line voting. In contrast, the new state-printed and distributed ballots relieved the parties of the critical task of distribution. They also made it possible for voters to split their votes among the parties and for the nominees to distinguish themselves from their fellow partisans (Rusk, 1970). Similarly, the direct primary broadened the nominating constituency in such a way that it too might be more tolerant of independent behavior.

I should also note in this connection that the Seventeenth Amendment (1913), which provided for the direct election of U.S. senators, had the same effect of weakening linkages among politicians. When state legislatures chose them, senators and would-be senators had to be involved in every aspect of state legislative activity, from their election to their internal organization (Rothman, 1966). Direct election freed one from the other. It also released state legislators from a highly partisan task which had induced some degree of partisan cohesion among them.

By 1920, with the adoption of womens' suffrage, the progressive reforms were in place. Between 1920 and the 1960s, minor adjustments were made in the states in matters such as electoral timing and in ballot forms. It was an essentially stable era in which office holders sought to reinforce their position. Most important were those changes that were *not* made. In 1920, for the first time, the Congress failed to follow the constitutional mandate to reapportion seats among the states with the new census. In state after state the legislatures followed suit in one way or another, failing to keep representative districts aligned with population growth in the newer metropolitan areas (Jewell, 1962). Firmly in control of the political machinery, officeholders were happy to use it to preserve their positions.

In sum, the new rules had created a structure of political opportunities that made it easier for individual party nuclei to go their own way within the parties at large. Note how this was accomplished in each of the three phases of the organizational cycle. In the nominating phase, direct primaries gave candidates a mechanism for gaining office free of the powers of slatemakers. During the electoral phase, the new ballot made it possible to create a campaign independent of the rest of the party. And in the Congress, the weakening of the party leadership reduced the pressure to cooperate within the governmental phase. To round out the potential for partisan fractionalization, the parties in Congress developed rules for the distribution of power which rewarded longevity in office rather than any demonstrated ability to unify the party in government or even to be minimally cooperative.

Here I must emphasize that the changes in the rules I have discussed did not in and of themselves create the disjointed party organization that developed in the first half of the century. The rules were a necessary or contributory condition leading to that end, but not sufficient in themselves. To explain why such parties developed, I must turn to the party system.

The Party System in the First Half of the Twentieth Century

As is widely recognized, the American party system underwent significant changes after 1896 (Burnham, 1970; Key, 1955). The states of the Deep South were to be completely dominated by the Democratic party as Republicans ceased even to compete. Large areas of the Midwest and the Northeast became almost as strongly committed to the Republicans (Schlesinger, 1955; 1960a, b). The Republican party immediately gained the dominant position in the presidency and the Congress, a position it lost only when its internal divisions allowed the Democrat Wilson to win the presidency in 1912. The general decline in competitiveness can be seen in the increasing margins by which elections were won. In the latter decades of the nineteenth century more than half of contests for major offices were won by less than 55% of the vote (Figure 1). After 1896 such marginal wins declined to less than 40% of most races.

In the present context the most significant point about the new system was that the changes in

Figure 1. Marginality in Elections
Percentage of Elections Winning Candidate Received Under 55% of the Vote

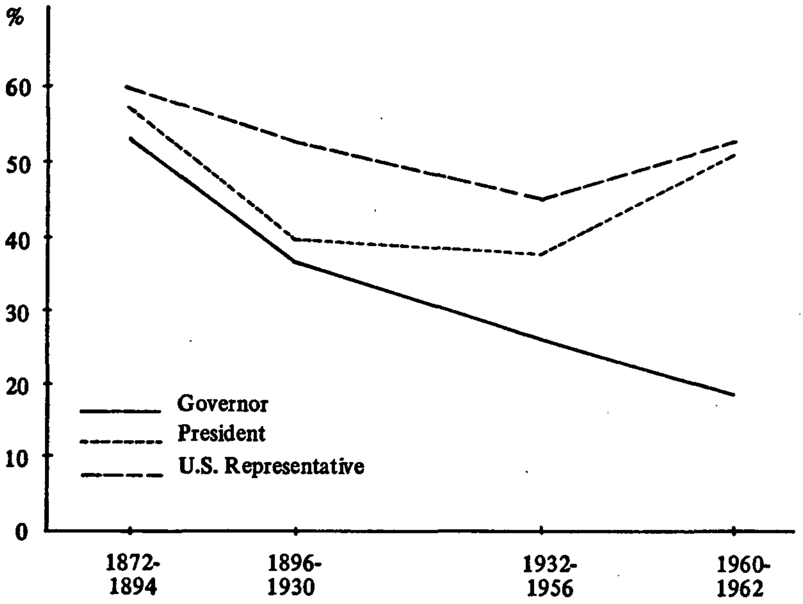


Figure 2. Percentage of Constituencies Which Had No Party Turnovers (One-Party Constituencies)

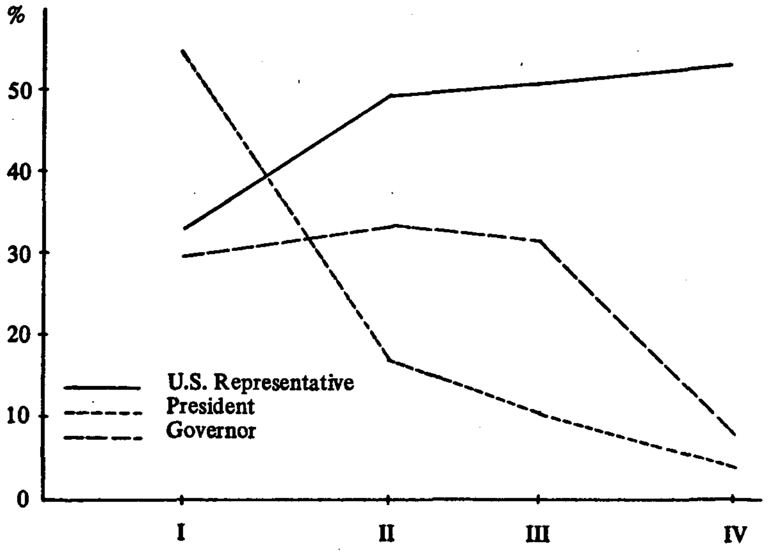
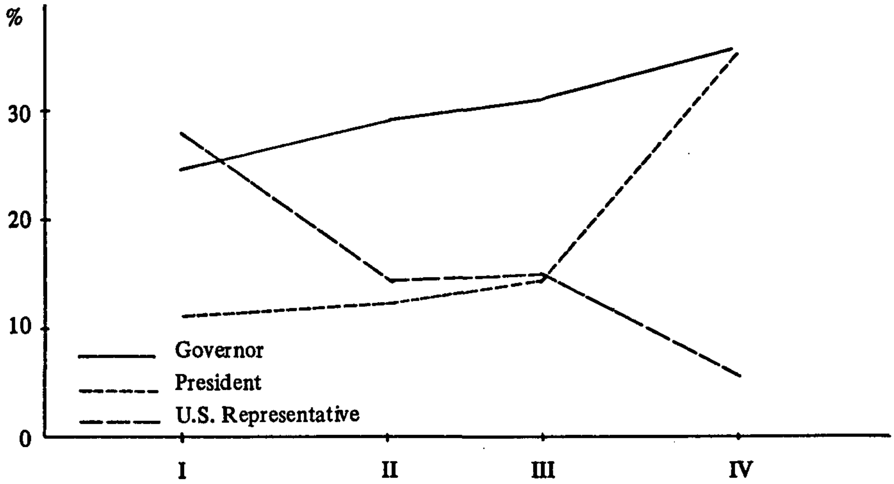


Figure 3. Percentage of Constituencies with Three or More Party Turnovers (Volatile Constituencies)



	President	Governor	U.S. Rep.
I	1880-1900	1880-1900	1882-1900
II	1908-1926	1910-1930	1912-1930
III	1932-1956	1930-1950	1932-1950
IV	1960-1980	1960-1982	1962-1980

(In the above, President refers to the vote in individual states.)

competitiveness were unevenly distributed among the offices; those with the broader constituencies—the presidency and statewide elective offices such as governor and senator—remained competitive, and in some respects became even more competitive over time. Offices with smaller constituencies—the Congress, state legislatures, and local offices—became increasingly dominated by one party. We can see some of this by observing party turnover in some offices in successive 20-year periods (Figures 2 and 3).[6] For the presidency, the majority of states never altered their vote for the parties during the latter part of the nineteenth century. In each successive generation during the twentieth century the proportion of one-party states has dropped. In contrast the proportion of noncompetitive congressional districts was only about a third at the end of the nineteenth century, a figure that has risen to over one half in the twentieth century. By the same measure we can see that the proportion of volatile constituencies (i.e., a turnover rate of three or more times in a 20-year period) has risen for president and governor, whereas it has steadily declined for the lower House of Congress.

The result was to sharpen the distinctions among the party systems around the various political offices. That the Congress and presidency should respond to different constituencies was, of course, part of the original master plan (Burns, 1963). The unevenly competitive system of the twentieth century did enlarge the gap. The mechanics of the Electoral College led to a weakening of the position of one-party states in the presidential nucleus as the parties vied for those votes that could be won in the competitive states. Thus the one-party character of the southern states cost them influence within the party system of the presidency. At the same time they gained power within the party system surrounding the Congress. Much the same can be said of the relationships between the party systems for governorships and those for state legislatures (Key, 1956, pp. 52-84). Malapportionment assured that control of the legislature represented a set of interests distinct from that leading to the governorship.

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[6] The calculations in party turnover over a 20-year period in congressional districts are inevitably complicated by the decennial reapportionment and redistricting of constituencies. We sought to retain as many districts as possible over each 20-year period. Some had to be dropped after ten years, whereas some were added after each reapportionment. The party affiliation of candidates are those listed in Congressional Quarterly's *Guide to U.S. Elections,* updated by relevant post election issues of the Congressional Quarterly *Weekly.* In those instances where party affiliations were unclear, they were checked against the listings in the *Biographical Directory of the American Congress, 1774-1961.*

How much of this separation of party systems was the result of the new rules? In part I can say that the new rules allowed the natural discrepancy between voting in large and small constituencies to develop. Large nuclei such as those for the presidency were incapable of adjusting their appeals to every state and legislative district. The smaller party nuclei could. Furthermore, the smaller and thus more homogeneous units were bound to be less volatile in their partisan responses. Thus by eliminating some of the structural links in the voting machinery, the rules of the twentieth century simply released what is a natural tendency toward disjuncture in a political structure such as that of the United States.

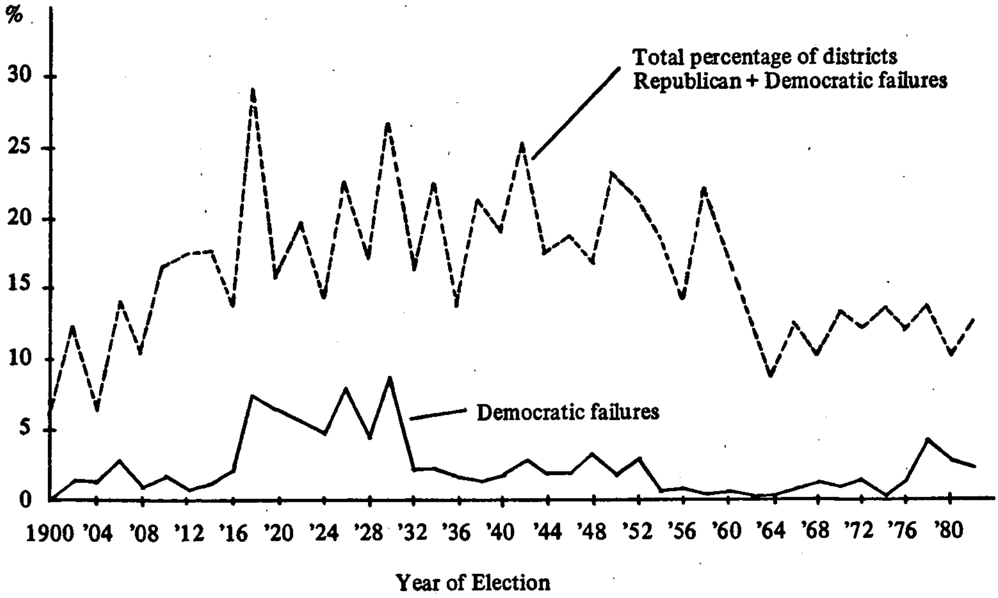
There were also aspects of the new rules which positively encouraged the decline in competition between the parties. One aspect was the new power structure in the Congress which made it profitable for constituencies to keep sending back the same representative time after time. Another was the primary nominating system, which had the effect of depriving the minor party in a constituency of its most precious resource, its monopoly of electoral opposition. The result was that conflicting ambitions and interests were able to fight it out, at least electorally, within the dominant party.

Yet to the extent that the separation of party systems resulted from a decline in the competitiveness of constituencies, it had to rest upon a growing rigidity in the behavior of the electorate. We know little about the strength of partisan identification before the development of systematic voter surveys. We do know that in the early 1950s approximately 75% of the electorate admitted to some degree of party identifiation, and half of these to strong identification (Abramson, 1983, chap. 7). The weakening of the level of competition in individual constituencies up to that time must have rested upon the domination of constituencies by the identifiers with one or the other party. Indeed, I would argue that the key to the party system of the first half of the century was the strength of partisan identification in the electorate, because once the nominations were made, it made the results of most elections quite predictable.

The Impact on Party Organization—The "Old" Party

As I noted in the outline of the theory of party organization, the level of competition critically affects all aspects of the party, the distribution of nuclei, the level of effort, and the linkages among nuclei; this was clearly evident in American parties as they had developed by the 1950s.

Figure 4. Congressional Elections Without a Candidate of One of the Major Parties (House of Representatives, 1900-1982)



The simplest test for the distribution of party nuclei is whether or not a party fields candidates for elective office. For the two major parties, getting a candidate's name on the ballot is a relatively simple task. The major problem is often getting someone willing to stand in the party's name. Certainly over large portions of the political territory at lower levels of office, only one party ends up with a candidate in the general election. For a significant office such as U.S. Representative, most constituencies find both a Democrat and a Republican running. Yet if we look over the time period of the first half of the century, we see, in accordance with the theory, a steady increase in the number of districts in which one party failed to have a candidate (Figure 4). From 1900, when only 22 districts had no two-party competition, the proportions rose, reaching a peak in 1918 when 128 districts, or nearly 30%, lacked major party competition. Over the first half of the century, the figures remained high, ranging around 20% of all districts.

The bulk of these failures to compete were, of course, by Republicans in the South. Yet, as can be seen from the time series, a significant number of Democratic nuclei failed to compete in the pre-New Deal era. In their areas of weakness, both parties tended to disappear.

What about the level of organizational effort? According to the theory of party, the less competitive constituencies are the less effort will be put forth by all the parties. By effort I mean simply activities aimed at getting candidates to run and then campaigning for them. On this there is no good general data to measure effort over time. Nevertheless, the impression one receives from regional studies and specialized studies is that of parties that were becoming increasingly empty vessels. V. O. Key's study of the southern states pointed up the emptiness of Democratic party organization there (Key, 1949, chap. 18). Candidates of course formed their own nuclear organizations aimed at capturing the party nomination, but once that was achieved, nothing more was needed, and nothing more done. In his later work on the non-southern states, Key used the term "atrophy" to describe much of party organization there (1956, chap. 6). In a systematic local survey, Eldersveld (1964, p. 442) found that as many as 44% of the voters in Detroit of the 1950s claimed never to have been approached in any way by a party, including through the distribution of campaign literature. In the study of four state legislatures in the same era, Wahlke et al. (1962, p. 98) showed that only in New Jersey did a majority of legislators mention their party as having sponsored their careers in any way. In Ohio, California, and Tennessee about one in four saw the party in this role. This was hardly an era of highly active party organizations.

In this connection I must note that the widely recognized decay of the urban political machine occurred during this era of the "old" party. Its decline is usually attributed to such things as the growth of the welfare state, which deprived it of its capacity to do favors for the depressed population of the cities and to the decline in immigration, which deprived it of its supply of dependent constituents. To that type of explanation I would add the general weakening of competition in the first half of the twentieth century. After 1932 the urban population became increasingly attached to the Democratic party. This was clearly the product of the appeal of the national organization; the local machines had nothing to do with it. The result was that machine organization became less and less useful to candidates and officeholders. The Democratic nomination was all that was needed. Once that became apparent, the machines were abandoned by their most important leaders, their candidates and officeholders.

My third concern is the multinuclear party and the linkages that develop among candidates and officeholders. According to the theory of party, such links should be strongest among competitive nuclei; thus we would expect a decline during the first half of the twentieth century. Here again there is little systematic evidence about most such links. How much cross-nuclear recruitment of candidates existed? Did candidates formulate joint campaigns? We know some things about monetary transfers, but we also know that reporting was very spotty (Heard, 1960). Caro (1981, p. 608) has shown how, in 1940, the inventive Lyndon Johnson, in a manner suggestive of modern PACs, gathered and distributed Texas oil money among Democratic congressional candidates. It was an era, however, in which campaign expenditures were relatively small and probably less important in deciding elections than they are today.

The best and most important indicator of party linkage occurs within the governmental phase. It is the degree to which party members vote with each other in legislatures, and the extent to which they align themselves with the party's leadership in the executive. Here we do have continuous evidence. In accordance with the theory, we should expect that as competitiveness weakened, so should the cohesiveness of the parties in government. This is demonstrated in Figure 5, which consists of the party cohesion scores by party for each Congress since 1900.[7] In the congresses of

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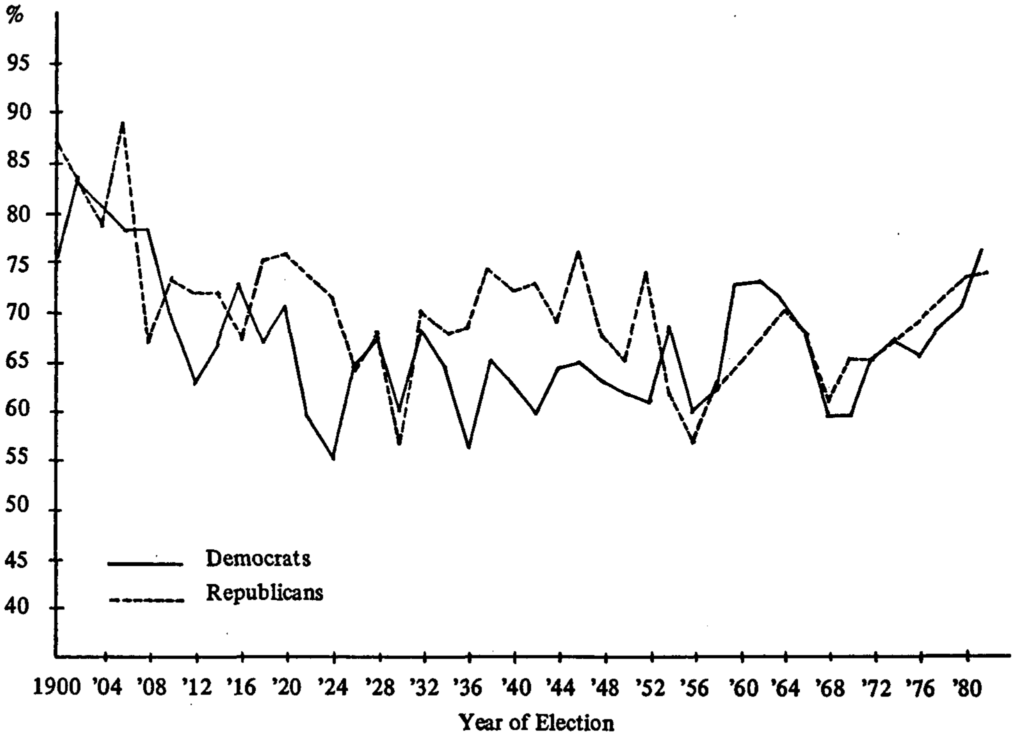
[7] Party affiliations of candidates in Figure 4 are based on Congressional Quarterly's *Guide to U.S. Elections.* In California, under cross-filing, and in New York, candidates sometimes appear as nominees of both the

Figure 5. Average Cohesion Scores, U.S. House of Representatives, 1900-1982



the pre-reform era, elected between 1900 and 1908, the figures show party line voting for both parties to be above 75%. After the new rules and with the decline in competition, cohesion went down in both parties, but most sharply within the Democrats.[8] Again the lower rate for the Democrats is readily explained by the fact that they encompassed the most independent nuclei, those elected in purely one-party districts. Also, the Democrats represented the most diverse constituencies. In neither party, however, did cohesion scores rise above 75% after 1920, with the exception of Republican cohesion after their 1946 victory which pitted them against the Democratic President Truman. Note that during the period of Democratic dominance of all branches after 1932, cohesion among House Democrats normally fell

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Republican and Democratic parties. In those cases I have assumed that their true party was the one with which they affiliated in Congress, and that the other party did not have a nominee. Since 1978 Louisiana has used a "nonpartisan" primary election, with a runoff election in the event that no candidate received a majority. I have assumed two-party competition to exist when both a Republican and Democratic candidate appeared in the first election.

[8] Some points should be made about the graph in Figure 5. The cohesion scores on which it is based represent the average level of agreement among party members on roll call votes. This is a different figure from the proportion of all votes that are party votes, that is, votes in which a majority of one party opposed a majority of the other. This latter figure is much more variable than the measure of party cohesion and appears to be greatly affected by such factors as divisions in party control between the Congress and the presidency as well as the existence of war. Thus I have chosen to emphasize the figure that measures the degree of agreement within each party. The figures from 1900 to 1966 are drawn from Brady, Cooper, & Hurley (1979). From 1968 to 1982 they are based on the annual figures in *Congressional Quarterly, Weekly Reports.* The Brady et al. figures are based on all roll call votes, whereas the *CQ* figures are based only on party votes, that is, those on which the majorities of each party were opposed. As Brady et al. note, their inclusion of all votes tends to overstate agreement within each party, because it includes unanimous votes. Thus the rise in internal party agreement, using the *CQ* figures, in the 1970s and '80s appears even more significant.

below an average of 65%. In the 1950s, when I would judge the old parties to have been most fully developed, the cohesion rate for both parties fell to below 60%.

I should also note here that in this era presidents had great difficulties in gaining the support of their parties in the Congress. Sizable victories such as that of Roosevelt in 1932 or, later, of Johnson in 1964 could bring in many new members, all from competitive districts and thus in need of all the support they could get. These members would provide the cooperative base to pass the president's program. Such outsize majorities would soon melt away, leaving the Congress in control of its members from the one-party constituencies. After the mid-thirties, the Congress was controlled for the most part by the nonpartisan coalition of southern Democrats and northern Republicans (Patterson, 1966). It was an outcome that rested firmly on the lack of competition in most districts.

In sum, a party system that had served to isolate constituencies from each other, in tandem with a structure of political opportunities that provided few automatic links among office holders produced what I consider a debilitated party organization. Although they remained as magnificent instruments for the development of political careers, the parties were weak as instruments for the making of public policy. It does not appear accidental that in the 1950s, when the realistic-behavioral school of political science emerged, that it focused on interest groups and "group theory" in explaining public policy.[9] That was where they saw the action to be. Partisan involvement in the making of public policy required the kinds of outsize majorities that Roosevelt and Johnson had at the beginning of their terms. But such policy formation had to be hurried action, because these partisan coalitions were soon lost. The old parties proved to be very weak as instruments for resolving the major difficult issues of the day. Thus the parties proved to be obstacles rather than solutions to the emerging crisis provided by American treatment of its black citizens. The major breakthroughs had to be made by the courts, the least democratic element of the system. Indeed the emergence of the courts as activist policy makers in such sensitive areas as civil rights and legislative apportionment is testimony to the inability of the parties to act, even in areas where there could be little question in which direction American values pointed. The other "winner" was the president, who was led to behave along the lines of the "Hamiltonian" model, as Burns (1965) has described it, the leader rising above parties and extending his powers to the limit.

The New Political Party

In the second half of the twentieth century a "new" party has been emerging, one that is more national in scope, more active, and with clear signs of greater linkage among its nuclei. This is true for both Republicans and Democrats. What has brought this about? A new wave of reforms in the rules of office seeking began to overtake the parties in the 1960s. However, in their anti-organizational thrust these rules were very much like those of the Progressive era. What has been different about this period in comparison with the earlier one is that an increasing instability in the electorate and thus a more competitive party system has accompanied these rules changes. As a result, in accordance with the theory of party, officeseekers have increasingly found organization in all of its facets more necessary. Let us examine each of the major variables.

The Structure of Political Opportunities

The most striking change in the rules has been the adoption of the peculiar state-by-state sequence of direct primaries for the selection of presidential candidates. The great increase in the number of states choosing their delegates by a direct primary followed upon the adoption by the Democrats of the multifaceted set of reforms recommended by its McGovern-Fraser commission for the 1972 nominations. The commission apparently did not set out to increase the number of primaries. Yet its directive to states to open up their parties proved to be most easily done by adopting a primary (Ranney, 1975, p. 206). Because in most states a primary required a state law, this development within the Democratic party affected the Republicans as well. There were, of course, many other regulations dealing with the characteristics of the delegates and with the ever-controversial issue of how primary votes are to be translated into numbers of convention delegates (Crotty, 1983). Some of these are of tactical importance to various types of candidacies and thus have been subject to quadrennial revision. The basic change is that presidential nominations today are made by primaries, albeit primaries that operate differently from those used by the states for other offices.

The overt effect of the presidential primary system is, however, much the same as the primary for other offices; it makes it possible for candidates to seek the presidency with a greater degree of independence from other elements of the party. For example, Jimmy Carter, with few ties to fellow Democrats, could win the nomination. The primary also makes plausible efforts to unseat an incumbent president, such as those made by Reagan in 1976 or Edward Kennedy in 1980. What the primary has not done is to reduce the need for political organization. Candidates must begin earlier to develop a staff that can devote full time to the complicated quest.

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[9] David Truman's *The Governmental Process* (1951) sparked the development of what came to be known as group theory. E. E. Schattschneider's *The Semi-Sovereign People* (1960) was a trenchant attack upon it.

What is new about these personal organizations is the target at which they are aimed, not the fact that they are personal. Aspirants for presidential nominations have always had to develop personal organizations in their search for delegates. Most presidential nominees had to contest for the honor. Franklin Roosevelt before 1932, or Thomas Dewey before 1944 or 1948 had thoroughly personal organizations devoted to their task of rounding up delegates. As the Democratic nomination in 1984 shows, nothing about the primary system precludes the development of linkages with other offices.

A second type of reform has been in the rules of voting. There has been a significant expansion of the potential electorate, first by the lowering of the voting age, but more important by assuring through federal law that black citizens in the South be guaranteed the right to vote. The effect of this on parties has been its impact on the party system. In particular, it could have a substantial effect on southern politics, because the Democratic party's status as the only party rested firmly upon the exclusion of the black voter.

The other major change in the voting rules has been the product of the one-man one-vote rulings of the Warren court during the 1960s. Requiring that legislative districts be equal in population, the rulings immediately brought into closer alignment the constituency bases of state legislatures with those of the governor; it did the same for the House of Representatives and the presidency. At the same time the strict application of the equal population requirement has served to weaken some of the geographic links that held nuclei together. Before the equal population rulings, districts often were constructed out of whole political units such as counties and towns. Thus there would be a readily perceived unity of interests among candidates seeking support from the same package of voters. The new rules tend to create unique constituencies, thus further separating officeseekers from each other. And since districts must be redrawn with each census, the new rules introduce a high level of instability into a constituency structure which in the past had been remarkably static.

The third area of reform has been in the rules on the financing of political campaigns. These too have the overt consequence of encouraging the independence of party nuclei. Whether that is much of a change from the past is not too certain. Candidates have always had to gather their own money. Some central disbursement undoubtedly occurred, but hardly enough to provide a tight control. However, through their requirements for strict accounting of how moneys are raised and spent, the new rules raise problems for any kind of joint effort, whether it be a common fund-raising dinner or the placement of the names of two candidates on a button. To which candidate's account are they to be allotted? The least complicated path is to keep all campaigns separate. At the same time accounting procedures do make more explicit and concrete those linkages that come through the raising and spending of money.

The high cost of campaigning today has made money gathering a central task for all political organizations. Both federal and state financial regulations have further imposed the need for complex and widespread organization as they set limits on the amounts that can come from a single source. Candidates must either create their own network or join some other. Even for those offices that enjoy some degree of public financing, the need for matching funds, again from small sources, means that candidates need a broad network of sources. The rules that have permitted political action committees to develop have, of course, spurred the growth of precisely this sort of money-gathering apparatus.[10] Here we would emphasize that nothing in this process by itself makes candidates more or less independent. That depends on how the process is used, which in turn, I argue, rests upon the party system.

The fourth area of reform has been within the Congress. There, several of the practices of the old era, which had concentrated power in the hands of a few senior members, had begun to break down even in the 1950s. In 1953 Lyndon Johnson, as Senate majority leader, adopted the rule that assured freshman senators of a choice committee assignment, thus weakening the monopoly of position by the elders (Ripley, 1969, p. 134). Within the House the major changes occurred during the 1970s. The critical reforms deprived the chairs of the committee of their very broad powers over agendas and procedures. Seniority became a less-than-absolute principle in the selection of committee chairs. Committees thus became internally democratic. In addition,

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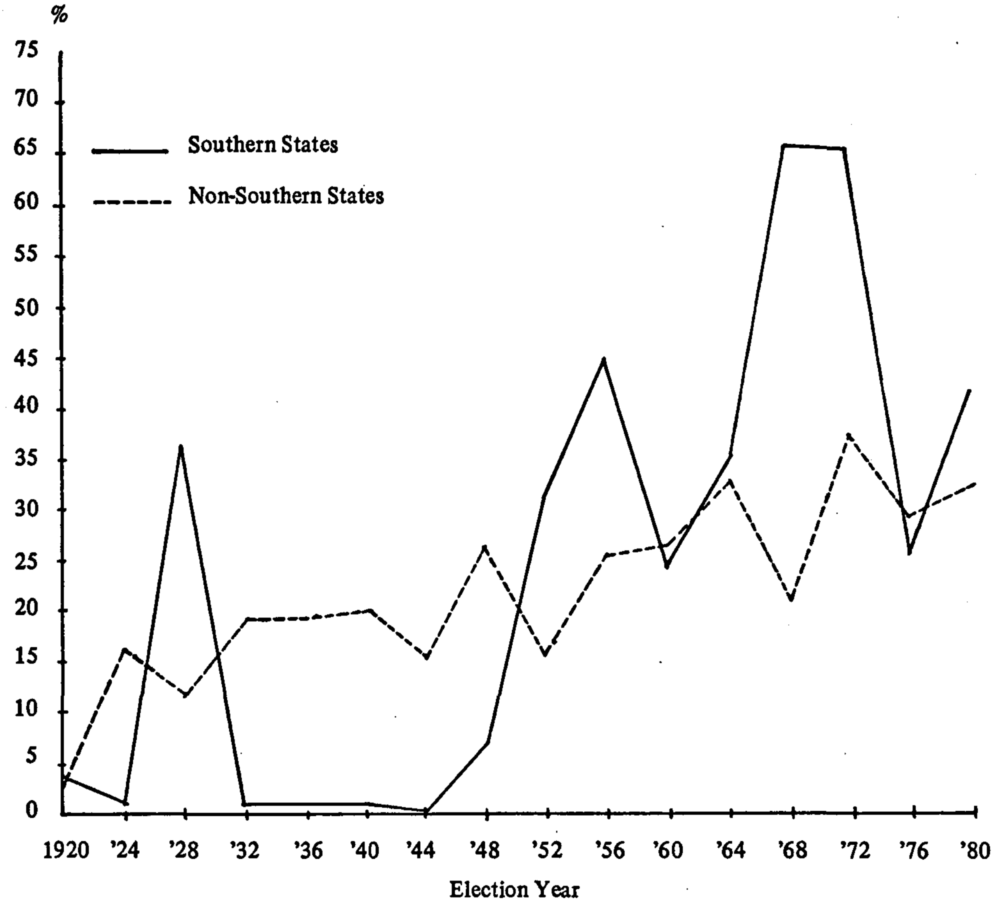
[10] The new campaign finance regulations have led to a voluminous literature. And the growth of PACs has attracted much attention. It is clear that PACs vary greatly among themselves and their tactics. Useful articles may be found in Malbin (1984) and especially that by Adamany (1984, pp. 70-121).

Figure 6. Percentage of Congressional Districts Voting for One Party for President and a Different Party for Congress



the number of subcommittees was expanded, thus enabling many junior members to gain a conspicuous position of power over a policy area (Ornstein & Rohde, 1978).

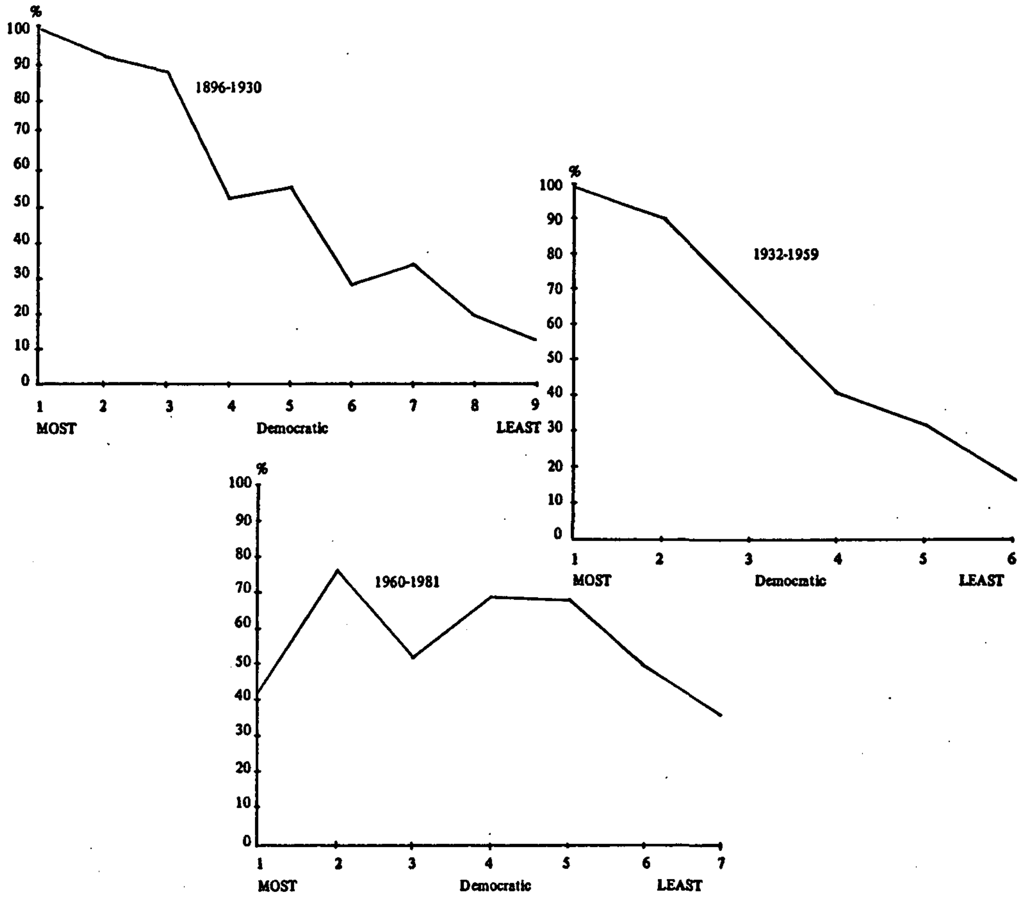
These changes within the Congress have generally been perceived as having made members more independent and thus as weakening the party leadership. For the most part, I would point out, the new rules were simply permissive. Whether or not members would act independently would depend on whether they thought that doing so would get them reelected, and that, I have argued throughout, depends on the party system.

I must note that the reduction in the powers of seniority was a positive step toward freeing the party to act in a unified manner. As the device for allocating power, seniority as often as not tended to frustrate party majorities. With the decline of the seniority leaders, the way was opened for greater influence by the party's elected leaders, its whips, caucus chairs, and the like. Whether they are able to exercise it again depends both upon their skills and even more upon the conditions in the party system which define the expectations of the membership. One consequence is clear. The party cohesion scores of committee chairs has risen markedly (Brandes & Hibbing, 1984).

The New Party System

On the surface the party system since 1950 looks much like its predecessor, differing primarily in the extent to which it magnifies the separatist tendencies in the American structure. Split-ticket voting, which was steadily rising from 1920 on, went to great heights during the 1970s

Figure 7. Percentage of Electoral Wins for Democratic Candidates for Governor Related to Scale Positions of States in Voting for President



(Figure 6).[11] The most conspicuous result has been the separation of the congressional and presidential party systems from each other. The Congress, and in particular the lower House, has become a Democratic bailiwick, whereas the presidency has become more competitive with a Republican tilt.

Another product of the voters' willingness to divide their support has been the separation of state party systems from the national party system. In the past a state that was Republican in its votes for president would also have Republican governors. Today this is much less likely to be true (Figure 7).[12] This separation in systems has been furthered in part by the fact that most governors today are elected in the presidential midterm year. Yet the actual divided party support requires flexibility on the part of the voters. Also within the states, even after the reapportionment decisions, there is as much division in party control between the governorship and state legislatures as there was before.

The most striking difference between the new and old party systems is that there are no states in which one party is completely unable to win any office, and I suspect that there are also relatively few constituencies of any kind that are as completely devoted to one party, as there were before the 1950s. This is a monumental difference, because what it means is that, despite all the separatist tendencies, the system as a whole is far more competitive than it was. It also means that there are more incentives for the growth of party nuclei in places where they never existed. The impact of this development on the distribution of party nuclei can be seen in the large drop in the 1960s in the numbers of uncontested congressional seats (Figure 4).

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[11] Cummings (1966, p. 32) gives the overall figures for split election results from 1920 to 1964. I have isolated the votes in the South from these in Figure 6 using Scammon (1965) for elections from 1920 to 1964, and successive post-election reports of *Congressional Quarterly* for subsequent elections.

[12] In Figure 7 the positions of the states with respect to the presidency have been defined for each period by Guttman-type scales which rank states from most to least Democratic, assuming consistency of voting behavior within each electoral era. The scales and their explanations are in Schlesinger (n.d.).

As can be seen in Figure 4, in presidential election years there are usually, although not always, fewer uncontested elections than in midterm years. That is as we should expect if the more intense competition and higher turnouts of presidential elections stimulate expectations at the lower level and if presidential nuclei find it useful to have candidates competing for the lesser office. Presidential candidates, however, may find it disadvantageous to have their party run candidates for the lesser office if that means that the opposition will be stirred up. In either case the linkage of nuclei is critical to the distribution of candidacies.

The new party system is thus something of a puzzle. Broadly speaking it has become much more competitive, as each party has developed a national geographic base. Old rules such as the malapportionment of legislative districts and restrictions on the suffrage have been eliminated. Thus the rigidities of both rules and attitudes which had limited the responsiveness of the old party system have been swept away. Yet the pieces of the system appear to be even more divided than in the past. If, then, the separation of elements of the party system I noted in the era after 1900 weakened the linkage among elements of the parties, today's even greater separation would appear to lead to still weaker parties, and of course that is precisely the argument of the decline-of-party thesis (Fiorina, 1980).

The problem with the above argument is that it works from the evidence of how constituencies vote rather than from an understanding of the reasons for that vote. It argues that straight-ticket voting unifies parties; split-ticket voting disunifies them. Straight or split-ticket voting can arise, however, for different reasons, and these have diametrically opposed consequences for party organization.

We need to distinguish between rigidities and flexibilities in the voting climate. Rigidities are factors that limit variations in voting behavior; they include structural factors such as ballots that induce straight party voting or attitudes, like a strong sense of party identification, which lead voters to vote always for the same party. Rigidities in attitudes express, by definition, long-run factors in voting, and thus exclude such short-run influences as the qualifications of the candidates or immediate issues. Flexibilities are the opposite: structural factors that permit variations and attitudes that emphasize short-run, immediate factors. The point that must be recognized is that rigidities and flexibilities can produce *either* straight or split partisan results in constituencies, and that it makes a world of difference to party organization whether rigid or flexible factors are the cause.

Flexibilities in the political climate are strictly neutral with respect to straight or split-ticket voting. A constituency totally devoid of people with any kind of long-run partisan attachment can vote a straight party ticket if all of that party's candidates appeal to them more than those of the opposing party. Some split-ticket voting would be expected inevitably in the American system because of the differences in constituencies. Candidates for president or governor must appeal to a national or statewide constituency. They cannot adjust their appeals to sectional differences; the parties in smaller constituencies can. Thus even in a totally flexible environment we would expect both straight and split-ticket voting.

Rigidities, on the other hand, appear at first to lead only to straight-party voting. Indeed, that is the case if voters cannot or will not divide their vote. On the other hand partial rigidities can lead to a great deal of ticket splitting, more even than will be found in flexible constituencies. This reasoning is prompted by the history of voting in congressional constituencies. Note Figure 6, where the degree of split-ticket voting for president and Congress is measured for elections since 1920. The southern states have, as we know, been the most rigid in their voting, particularly for lower offices. In the period between 1920 and 1948, with the exception of 1928, only a trace of southern constituencies showed any divided vote. Non-southern states range around 20% of the constituencies with a divided vote. After southern voters found that they did not have to vote for Democrats for president, the proportion of divided constituencies in the South soared, reaching heights of 65% or more in the elections of 1968 and 1972. Divided voting has certainly risen in the non-southern states as well, but only by about 10 percentage points. In the earlier period rigidities in the South depressed the national figures on split-ticket voting; since 1950 these same rigidities have elevated them.[13]

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[13] The figures on ticket splitting in Figure 6 probably understate the level of such split voting in the period before the 1950s. The election of 1952 was the first one

What difference does it make whether straight or split-ticket voting arises from flexible or rigid attitudes in the electorate? It is important because the more rigid the electorate, the more it frees the separate nuclei from each other. Straight party voting in that instance provides no pressure for multinuclear cooperation—which of necessity is immediate. Straight-party voting based on long-run attachments weakens party organization in all three of its facets—its distribution, its level of activity, and its linkages.

In contrast, the more flexible the electorate, the more it must be addressed in terms of its current concerns. Each nucleus appealing to a constituency must present its own case; its candidates, its campaigns, its issues, its ability to build up a base of support—all of these things are constantly at stake. The more flexible the electorate (i.e., the more competitive), the more does organization in all its facets become valuable. Candidates cannot rely upon partisan identification to gain election. Barring that, candidates must do things, get people to work for them, make promises, that is, create an active organization. At the same time the more flexible are electorates, the more other nuclei need the same sort of help, and thus the more each nucleus can do for each other. Competitiveness or flexibility thus is the key to organizational development.

Now we can see why the party systems in which the old parties functioned produced weaknesses in the structure of party organization, whereas the party systems in which the new parties must work produces strengthened linkages. The more flexible the electorate, the less secure can any candidate be. There can be no safe margins in a flexible electorate in the same sense that there are safe margins in a rigid electorate. Indeed the more rigid the electorate, the more all margins are safe; flexible electorates make all margins unsafe.

Thus the essential difference between the old and new party systems resides in the altered attitudes and behavior of the voters. If we can exaggerate for the sake of argument, the electorate of the 1950s had emerged as one strongly identified with one or the other party, an electorate that voted consistently and voted straight party tickets. This strong partisan identification in the electorate undermined all of the reasons why politicians organize. Since the 1950s partisan identification has eroded. It remains a potent force, but it has eroded enough so that almost all constituencies in the United States have shown themselves capable of voting Republican or Democratic for some office. This fact has not been lost on politicians. As long as a Democrat can become governor of Utah, Democratic candidates for president will think it possible that they too might win there under the proper conditions. As long as Republican candidates for president can win in congressional districts in South Carolina, Republicans in those areas can aspire with some reason to be congressmen or state representatives. Thus it is the very weakness of partisan identification among the voters which is a stimulus for the growth of partisan organization. It holds out hopes in situations that otherwise would have appeared hopeless.

Today incumbent congressmen can gain reelection by electoral margins that would be considered safe by any historical standard, but this does not appear to have heightened their sense of security. Close observation of congressmen by Fenno (1978) demonstrates a high level of insecurity. That insecurity is based upon the new reality. In the old party system, in which the party label assured the great majority of the votes, a member of Congress could feel secure with a 60% electoral margin. In the new party system, that member knows that those votes can be transferred. They must organize to hold on to those votes (Garand & Gross, 1984). It does not appear accidental that the growth of congressional staffs, the concentration upon constituency services, and the near-continuous campaigning exhibited by most members of Congress also coincides with the loosening up of partisan attitudes among the electorate.

Certainly the reshaping of the internal power structure of the Congress appears to be related to the increased need of members of Congress to be able to achieve prestige that they can use back home. The simple fact is that the old system of waiting one's turn for power is a weak weapon with which to fend off electoral attacks. Insecurity, as well as the desire for power and position, was important in pushing through those reforms of the 1970s which have spread powers among more subcommittees, and reduced the domination of committee chairs and once all-powerful committees such as Rules and Ways and Means.

The critical characteristic of a competitive party system is insecurity, and that is precisely what the new party system has in great amounts for candidates and officeholders. In accordance with the theory of party, I would expect all aspects of party organization therefore to develop more fully. As I have already noted, the new parties, unlike their

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in which election returns were broken down by congressional districts. Approximately 100 districts thus are missing from these earlier calculations. All of these are non-southern districts, and all must be in metropolitan areas where we would expect ticket splitting to be higher than in the more rural areas on which the data are available. If this inference is correct, then practically all of the increase in ticket splitting since the 1950s is a product of the changes in southern voting.

predecessors, are national parties in that they have nuclei in every region and state. Certainly the level of organizational activity has expanded greatly over the past two decades. This is true whether one takes a circumscribed view of the organization to encompass just the formal apparatus, or our broader view, which includes all efforts at office under the party label. We are indebted to the wide-ranging studies of the party apparatus by Gibson and colleagues (1983, 1985) for firm evidence of the growth of state and local organizations over this period. More organizational effort leads to more intensely competitive contests, which in turn make organizational effort ever more worthwhile.

Linkages within the parties have also become stronger. We have no measure of the extent to which cross-nuclear efforts are made to recruit candidates and provide resources. Our impression, however, is that far more of this activity goes on today than was the case in the old party. Political action committees, the new organizational device, will, I predict, have an increasingly partisan role to play. Certainly the exigencies of the ways Congress makes policy require that any business or group concerned with a specific policy ignores party as it seeks to gain favor with incumbents in key positions to affect that policy. Groups with broader policy concerns, however, and that includes the same businesses and single-interest groups, must follow the lines held out to them by the competitive party system. Ideological groups and their PACs are clearly partisan and are providing an increasingly professional and sophisticated linkage among candidates.

The ultimate test of party linkage is the behavior of the party member in government. Here the figures within the lower House of Congress show striking increases in party line voting (Figure 5). For the Democrats in the House from 1920 on until the 1960s, the era when the old party was in full flower, the party's cohesion rate rarely rose above 65%. As the more homogenous party the Republicans usually scored somewhat higher. In the period of the 1960s cohesion within both parties dipped, undoubtedly affected by the twin traumas of Vietnam and Watergate, both of which split the parties. Since 1968, however, there has been a rise in cohesion scores for both parties. Indeed both parties look more like each other in rate of cohesion than they have before. In 1983 the Democratic party reached a cohesion level of 76%, a degree of unity in voting we have to go back to 1908 to find in the history of the party. This is truly remarkable for a party lacking the earlier mechanisms of discipline, and for a party that we are so often told is coming apart. The greater cohesiveness of American parties is exactly what the theory of parties predicts should result from increasing levels of competition.

Thanks to increasing levels of competition between the parties, then, American political parties are stronger than before. Not only has the spread of competition to all regions meant greater voting cohesion within the Congress; it has also reduced the range of ideological differences within the parties. Liberal Republican congressmen like Donald Riegle of Michigan and John Lindsay of New York came to find the Democratic party a more congenial home. Conservative Democratic congressmen such as Phil Gramm of Texas and Andrew Ireland of Florida follow Strom Thurmond's path to the Republican party. Finally, as Senator Richard Lugar (1984) pointed out in a recent appeal for funds for Republican lawmakers: "There is no such thing as a 'safe' seat anymore." Candidates need all the help they can get; they are finding that the best place to get it is from their fellow partisans.

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Communications

Comment on Claggett, Flanigan, and Zingale (Vol. 78, March, 1984, pp. 77-91)

Claggett, Flanigan, and Zingale find no trend toward nationalization of electoral responses, which challenges Stokes's findings of such a trend. How can we reconcile such different findings? We suspect the varying results may be a function of some significant methodological differences which create considerable doubt about the validity of the former's findings.

The first problem is the theoretical issue of choosing counties as the unit of analysis rather than congressional districts. The argument regarding nationalization focuses on whether legislators are able to go against national electoral patterns. Individual legislators, not counties, cultivate districts, exploit incumbency, and try to run independently of broad national partisan trends. Using counties loses this focus and is theoretically questionable.

Table 1. Distribution of Counties by Number of Districts

Number of Districts	Frequency
1	2,767
2	221
3	50
4	20
5	6
6	2
7	1
14	1
17	1
Total	3,069

Table 2. Number of Counties per Congressional District[a]

Counties	Frequency
1	84
2-5	150
6-10	69
11-20	79
21-50	35
50+	2

[a] Total number of districts does not sum up to 435 owing to the exclusion of Alaska, Delaware, North Dakota, South Dakota, Vermont, and Wyoming, all of which have only one congressional seat. Virginia (10 seats) was also excluded because it contains a number of independent cities. It is unclear how the data are reported for these cities.

Source: *Congressional Districts in the 1980s.* Washington, D.C.: Congressional Quarterly, 1983.

This theoretical uncertainty is complicated in that many counties encompass multiple congressional districts. They recognize this problem, but assert that only "a few counties have more than one congressional district" (p. 85). Data from the current situation indicate this has become far more pervasive than they acknowledge. Table 1 presents the distribution of counties according to the number of districts contained either wholly or partly within a single county. Roughly 10% of the counties have this situation, such that reactions to many candidates are producing electoral results within a county. This certainty clouds the meaning of election results in ways that are unclear.

The most significant impact of using counties may be on the extent and structuring of variance. Substituting components of district scores (county scores) increases enormously the number of units of analysis and magnifies local variance in any given year. Currently the number of cases changes from 435 to 3069. To the extent county scores have greater variability (which is virtually certain), this substitution creates a data set in which local variance is heavily exaggerated. Table 2 indicates the distribution of districts according to the number of counties within a district for the 1980s. Using their approach, change scores for 2,262 counties replace change scores for 125 districts (of 419 possible). Roughly 75% of the units of analysis are representing change in only 30% of the congressional districts.

If county scores are more variable, local variance increases, as Table 3 illustrates. The scores represent change. Variance is calculated using the conventional approach of squaring district scores to give the total variance. Deviations and correspondence with national changes are squared to give their respective proportions of variance. The proportion of variance "attributable" to a national pattern is shown to the right. The national component declines as variability increases and the uniformity of movement with the national average declines. If any one of these scores is replaced by a set of scores with greater variability, the national component declines. The effect of such a substitution is shown in line 5 of Table 3.

The scores of line 2 are 4.0, 6.0, and 8.0. In place of the score of 4.0 the scores of 0.0, 2.0, 4.0, 6.0, and 8.0 are substituted. The national variance component declines from 93 to 75%. We cannot offer empirical evidence of the greater variability

Table 3. National Variance Components with Varying Data Sets

Data set scores			National average	National variance	Total variance	Nationalization
6.0	6.0	6.0	6.0	108	108	100%
4.0	6.0	8.0	6.0	108	116	93%
2.0	6.0	10.0	6.0	108	140	77%
0.0	6.0	12.0	6.0	108	180	60%
0.0	2.0	4.0				
6.0	8.0	6.0	4.0	164	220	75%
8.0						

county scores have from 1840 to 1970, but our spot checks of various districts showed a regular pattern of considerable within-district variation, such that using counties systematically increases local variance. Because in many cases a single district score is replaced by 20 plus county scores, the effect could be substantial.

Does this have implications for trend assessment? Claggett et al. acknowledge this problem with local variance but remain "less clear" (p. 85) about its effect on trend. We suspect the national trend may be significantly affected. As the number of states has increased over time, so has the number of counties. We wonder if across time the substitution of counties with their greater numbers and variability has overwhelmed whatever nationalization might exist. It needs to be demonstrated that this isn't relevant before discarding Stokes's analysis.

Urbanization accentuates this problem. Over time the population has increasingly concentrated in relatively fewer counties. Because congressional seats have generally been apportioned on the basis of population, this has probably produced relatively more counties (cases) containing more than one district and relatively more districts composed of a larger number of rural counties (cases) to offset the urban counties.

Finally, including noncontested races and southern states increases local variance in another way. A count of noncontested races showed a dramatic increase after 1900, with almost all in the South. When such races follow each other, as was often the case, the change score is 0, which is likely to increase local variance. When a noncontested race is followed by a contested race, the change is likely to be erratic and much different from the national change, again increasing local variance. (Furthermore, it is hard to imagine theoretically justifying these practices.) The impact of this decision is magnified in that southern states average more counties per state (the 12 southern states average 101.7 whereas the remainder average 74.3). The effect of noncontested races on the overall data set and on creating more (odd) local variance is increased when it is the southern states that have such races. This might well reduce the appearance of nationalization after 1900.

The South represents a final major problem. It has long had an electoral system insulated from national partisan pressures. Including voting changes in Southern *general elections* means including votes not tied to national issues for most of the period from 1840 to 1970. The effect of this erratic variance on the whole data set is considerable since the 12 southern states have 1220 counties (39.8% of total). The proportional dominance of these states in the data set has diminished over time (with more states), but the Republican party only got started in the South in the 1960s, and the increase in local variance from including these races was certainly still significant up to 1970.

We do not know if there has been any nationalization of the American electorate. We also are unsure of the exact impact of these problems, although it does appear that all their decisions have increased local variance. We do find too many unknown impacts. It appears that the ambiguities and problems of using counties preclude using their analysis to draw any conclusions regarding the nationalization of the electorate.

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Reply

Carrothers and Stonecash suggest that our failure to find an upward trend in the nationalization of the partisan division of the congressional vote, contrary to Stokes's assertions (Stokes, 1967, p. 196, cited in our article, p. 80), results from our use of counties as subunits rather than congressional districts.

The first and most important point to be made in response is that the basic finding of no upward trend in the national effect does not depend in any way on the choice of subunit. Subunit data did not enter this part of the analysis. Following Stokes's method, the calculation is based simply on the variation in the national percentage of the vote. As we said in the article: "In both Stokes's analysis and in ours, the national effect is calculated from the actual nationwide election percentage and is, therefore, unaffected by the choice of subunit" (p. 85). Thus the choice of subunit is irrelevant to the calculation of the national effect displayed in Figure 2 of our article (p. 79).

The discrepancy of interest is not between Stokes's calculations and ours, but rather between the pattern in turnout which Stokes displayed in full and the pattern in partisan voting for which he displayed only the declining constituency effect. The first and perhaps most significant point in the article is that a precise replication of Stokes's national effect associated with the partisan division of the congressional vote does not demonstrate an increasing value over the years. The low, steady national effect for partisan voting in Figure 2 (p. 79) contrasts dramatically with the increasing value of the national effect for turnout reported by Stokes (1967 and shown in his Figure 3, p. 194). With the exception of the decade of 1912 to 1920, when our calculation of the national effect is quite large, the national variance components for the partisan division of the vote are generally below 10.0 and never half as large as the constituency effect. This, of course, is in marked contrast to the national effect for turnout found by Stokes for the same years which showed a sharply increasing "nationalization" pattern during the twentieth century (Stokes, 1967, Figure 3, p. 194). Again, none of these observations depends on using data different from Stokes's. We simply added to the data already presented by him a national effect calculated as he would have done it.

Despite Stokes's original caution about the extent of nationalization in the partisan division of the vote (Stokes, 1965, p. 78), his later writing (1967) clearly suggests increased nationalization in both turnout and partisan voting, and it is this apparently erroneous conclusion that our article attempted to dispel.

Stokes also presented some analysis that "normalized" these effects, and Carrothers and Stonecash raise the possibility that a normalized national effect for the partisan division of the congressional vote as calculated by Stokes might demonstrate increasing nationalization. As they point out, this could occur because the lower level effects would be declining and the national effect would represent a larger and larger share of the total variation over the years.

Although we did not carry out this calculation in our article, with one modest assumption it is possible to estimate the normalized national effect from the data available in Figure 2 of our article; that is, Stokes's constituency effect and our calculation of his national effect. If Stokes' estimates of state effects are assumed to remain steady and low throughout the time period, they can be ignored in the normalization and we can calculate a normalized national effect based on the sum of the national and constituency effect. It is reasonable to suppose that state effects would not be large since they are generally low in the turnout estimates for the whole period (Stokes, 1967, p. 194), and the state effect for partisan voting from 1952 to 1960 is quite small (Stokes, 1967, p. 186). The normalized figures estimated in this way show no substantial nationalization in the partisan division of the vote. At the end of the series the normalized national effect is a little more than one-third of the constituency effect and the normalized national effect never shows the steep increase that was observed with turnout. (The normalized national effect for the partisan division of the vote, calculated with constituency effects but without state effects, are as follows: 1870s = .19; 1880s = .21; 1890s = .26; 1900s = .21; 1910s = .56; 1920s = .13; 1930s = .31; 1940s = .40; 1950s = .40.) By contrast, in the normalized turnout series presented by Stokes, the last two decades yielded national effects over 80% (Stokes, 1967, p. 194). It should be repeated that we have used Stokes's calculation of constituency effects and our calculation of the national effect (following his method); thus these normalized estimates do not substitute county subunits for congressional districts.

Throughout their note Carrothers and Stonecash indicate concern about the substitution of counties for congressional districts as units of analysis which we introduced in the last part of our article. We can appreciate their concerns because we had the same reservations. We therefore took some straightforward precautions in our analysis and these show that the counties do not introduce the anticipated biases into the over-time patterns. For example, we ran all the various forms of analysis weighted for population size as well as unweighted without affecting the estimates of the several effects over time. We also ran the analysis separately for the non-South because of the possible bias introduced by the unusual characteristics of the South and its uncontested congressional elections during much of the period. None of these variations produced results that undermined the conclusion that with respect to the partisan division of the congressional vote there is no clear pattern of increasing nationalization over the past 100 years.

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What if "Fools Die"?

A Comment on Axelrod

(Vol. 75, June 1981, pp. 306-318)

Robert Axelrod's article, "The Emergence of Cooperation among Egoists," and the companion piece written with William D. Hamilton that appeared in *Science* (Vol. 211, 27 March 1981, 1390-1396) have received well-deserved attention in political science and related disciplines. If cooperation can emerge as a result of the exercise of individual rationality, then Axelrod offers a solution to the dilemma posed by Thomas Hobbes in his *Leviathan*; that is, a solution to the "war of all against all" can emerge that does not require the imposition of an all-powerful sovereign. Recently, however, I attempted to develop a lecture based on Axelrod's work, and was interested in exploring the sensitivity of his results to changes in the values of the payoffs in the Prisoner's Dilemma game. With apologies to Mario Puzo for the title of this comment, I would like to share two insights with the readers of this *Review.*

In his 1981 *APSR* article, Axelrod offers and proves Theorem 2. To wit, "*TIT FOR TAT* is a collectively stable strategy if and only if

$$w \geqslant \max\left(\frac{T-R}{T-P}, \frac{T-R}{R-S}\right)\text{" (Axelrod, 1981, p. 311).}$$

Suppose, as Puzo writes, that "Fools Die"; that is, suppose that the "sucker payoff" is death, or some other suitably horrible reward. Does this change Axelrod's result? Intuitively, one would seem less likely to play *TIT FOR TAT* if the *TAT* was a particularly nasty one. However, Axelrod's result is unchanged, even should the sucker payoff be negative infinity. In that case, the first term in the maximand is much larger than the second and therefore defines the value of the parameter, *w.* By Theorem 2, *TIT FOR TAT* is a collectively stable strategy regardless of the value of *S,* the sucker payoff.

Later in the same article, Axelrod considers the possibility of "clusters" of cooperating individuals "invading" a group of defectors. He says, "The striking thing is just how easy invasion of ALL D by clusters can be" (p. 315), and goes on to show that invasion can occur when the cooperators interact with one another only one time in 21. "In this way (cooperators interacting with one another only infrequently), a world of meanies can be invaded by a cluster of *TIT FOR TAT*—and rather easily at that" (p. 315). He illustrates this with the example of cooperating business students going forth to compete in the world of business. Their willingness to cooperate with one another would lead them to "find that their lessons paid off" (p. 316), presumably enabling them to take over major corporations from incumbent meanies.

A bit of calculation will show that the ease at which cooperating clusters can invade *ALL D* depends critically upon the value of *S,* the sucker payoff. In Axelrod's example computations, the value of *S* is zero, with the result that the required frequency of interactions among cooperators is one in 21 (from equation 3 on p. 315). Suppose, however, maintaining other parameters the same as in Axelrod's example, that the value of *S* were negative 10. If so, the frequency of interaction between cooperators would need to be greater than one in three for invasion to succeed. If the value of *S* were negative 100, the frequency of cooperating interactions would need to be approximately 5 out of 6. If *S* were as large as negative 1,000, virtually all interactions would have to be with like-minded cooperators for *TIT FOR TAT* to invade *ALL D.*

These two points cast some doubt on the robustness of Axelrod's solution to Hobbes's dilemma. On the other hand, they may help to explain some observable phenomena. Some human groupings have been known to kill all members of another group who attempted invasion. Perhaps they, in some fashion, understood the logic of

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I am indebted to Robert Axelrod for his helpful suggestions on an earlier draft of this Comment.

Axelrod's analysis. Also, I have never been struck by the prevalence of happy cooperators in businesses I have known (nor in academic circles, for that matter). Perhaps invasion of the meanies by cooperators is more difficult than Axelrod would have it.

The point of this comment, other than to have some fun, is to suggest that we should conduct analyses of the sensitivity of our results to the values of each of our parameters. Axelrod analyzes the sensitivity of his clustering result to changes in the value of his interaction duration parameter, *w*, and shows that invasion by cooperators remains relatively easy (p. 316). Had he conducted a similar sensitivity analysis on the effects of changing the sucker payoff, *S*, he might have concluded that cooperative invasion is not so easy after all.

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Comment on Cain, Ferejohn, and Fiorina (Vol. 78, March 1984, pp. 110-125)

A distinctive feature of the British political system is that members of Parliament deal personally with the individual and collective problems of their constituents. The most assiduous MPs spend endless hours listening to complaints, making phone calls, visiting local institutions, writing letters and acting as facilitators with local civil servants and local authorities. Why do they do it? Cain, Ferejohn, and Fiorina believe that they do it largely for the votes, that electoral incentives are an important motivation in performing constituency service. I shall argue that: a) electoral incentives are present but not as important as the authors imply, and, b) the standard explanation, "sense of duty," is much more plausible than the authors admit.

In a valuable comparative analysis, Cain, Ferejohn, and Fiorina have shown that the electoral effects of constituency service are larger than anticipated, and, in another article, they have shown that the greater the need for votes (i.e., marginality), the greater the caework effort (Cain, Ferejohn, & Fiorina, 1983). But the results are weak, and the results of a similar analysis, reported in Table 1, are weaker still and statistically insignificant. These data are based on interviews conducted in 1972-1973 with 338 backbenchers (Searing, 1982). The dependent variable, "time in constituency," was estimated by respondents in terms of hours for an average week. "Marginality" is the difference between the winner's percentage of the total vote and that of the candidate who came second. "Tenure" measures length of service in the House of Commons. "Role" partitions respondents into those for whom constituency service is the principal career interest ("Constituency Members") and those for whom it is not. "Party" denotes Conservative or Labour affiliations. "Distance" measures the constituency's distance from the House of Commons. And "Security" enters the political value with closest ties to constituency service (Searing, 1978). The standardized regression coefficients listed in Table 1 show the impact of each independent variable upon time spent in constituency when the remaining independent variables are statistically controlled. It can readily be seen that "Marginality" faces in the proper direction, but that it has no significant impact upon the time that backbenchers spend in their constituencies.[1]

Table 1. Regression of "Time in Constituency" on a Selected Set of Independent Variables

Independent variables	Beta	*F* ratio
Marginality	-.083	(1.37)
Tenure	-.182	(6.91)**
Role	.230	(11.44)***
Party	.079	(1.15)
Distance	-.023	(0.11)
Security	.008	(0.02)
R^2	.106	

**Significant at .01.
***Significant at .001.

Why are these results weaker than those of Cain, Ferejohn, and Fiorina? One possibility is that if their data included frontbenchers as well as backbenchers, this might have artificially boosted the relationship, because, compared to backbenchers, frontbenchers tend to have less marginal constituencies and much less time to spend there. On the other hand, Cain, Ferejohn, and Fiorina may be on target, and had we both used better measures (MPs' own estimates of their electoral insecurity) my data might have shown the weak but significant relationship and their data a better one. The third possibility is that both analyses are correct, but that constituency work has become increasingly linked to electoral incentives between 1972-1973, when I collected my data, and 1978-1979 when they collected theirs. Speculation aside, however, the results we have at hand are mixed and weak, as they have usually been on this topic even in the United States where one would expect to see the strongest correlations (Johannes, 1983). Cain, Ferejohn, and Fiorina have shown that electoral incentives motivate some MPs, that British commentators underestimate their role, and that such motives may be becoming more common as party ties weaken. But there is no convincing evidence whatsoever to support the view that electoral incentives are anything like primary motivations behind constituency service. Because electorates still vote for parties rather than for individual candidates, the "personal vote," the vote that can be affected by constituency service, is very small—too small to make enough of a difference for enough MPs to make its pursuit a generally important motive underlying constituency service.

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[1] Because this result could be affected by extreme cases on the dependent variable, the regression was re-run with these cases re-coded. There was no change in the outcome. Similarly, because the distance variable plays an important part in role choice, the regression was rerun introducing "Role X Distance" as an interaction term. Again, there was no change.

There are other incentives involved. MPs themselves say they do it because it is "part of the job," part "of my unwritten contract of unemployment." They say they do it largely out of a "sense of duty," the standard explanation for constituency service. Cain, Ferejohn, and Fiorina tend to dismiss these claims, not with evidence but rather with a "they-would-say-that-wouldn't-they? attitude which suggests that such statements are rationalizations for other motivations. Perhaps so. Nevertheless, the claims deserve serious investigation, because in Britain constituency service has a special status which makes "duty" a very plausible incentive indeed.

Thus, constituency service is interwoven with British concepts of representative and responsible government. It is interpreted as redress of grievances, a mode of representation that helps to promote legitimacy and to check an executive that is subject to very few formal restrictions on its powers. Redress of grievances is the essence of constituency representation in Britain, for the representation of the public's political opinions takes place at the national level and is not regarded as the proper function of the constituency MP. Instead, representing constituents by making representations on their behalf to central government and other authorities has always been a central aspect of the MP's duties. Redress was, in fact, parliament's original function and was for centuries the only function performed by representatives. Medieval theory cast the role by characterizing members of parliament as attorneys for their constituencies, as agents whose job was to protect and advance the interests of the represented (Jolliffe, 1937). This function has been revitalized in response to the growth of central government and the complexities of the welfare state. Hence, when MPs explain that "you don't do things to put people to support you; you do it because it's your job and your duty to help everybody," their claim should not be rejected prematurely, particularly because it is not inconsistent with available evidence. It should be regarded as a plausible hypothesis, particularly because this duty is embedded in ancient institutional and normative features of the British political system. Perhaps the overall weakness of models used to explain constituency service arises from our tendency to overlook incentives such as "sense of duty" which may be more difficult to measure than electoral insecurity but which are certainly no less rational.

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Reply

Donald Searing's thoughtful letter warrants a response, although we believe there is less disagreement between us than he implies. Searing generally objects to any claim that the primary motivation for constituency service among British MPs is electoral, or more broadly as political, contending instead that service is part of the job description, something MPs do because they see it as their duty. To the best of our recollection we have never argued that electoral considerations were the primary motivation for constituency service. We did state that members believe that service activities have an impact (1984, p. 115). We did find that constituency reputation and activities actually have a marginal, although significant electoral impact. And in the *CPS* article referenced by Searing we did find that MPs who were least electorally secure took the most interest in constituency work. As noted by Searing we did express skepticism about the notion that electoral considerations and constituency work are totally unrelated, as some have suggested, but we did not go to the other extreme and argue that electoral gain was the principal motive for serving the constituency.

As stated in our article (p. 116) three questions often are confused. Do politicians believe a particular activity has an electoral impact? Does that activity have that impact? Is that why politicians do it? Our research supports positive answers to the first two questions with respect to constituency service in the United States and Great Britain. The third question, however, is one that can be approached only indirectly. We must develop hypotheses premised on the assumption of electoral motivation and see how well they fit the data. Thus, in our *CPS* article we looked to see if more marginal members show higher levels of constituency effort. They do. This is consistent with an electoral motivation, to be sure, but not necessarily inconsistent with other motivations, and in no way does such a finding imply that only electoral considerations matter. On the general argument we simply do not believe that Searing has any major quarrel with us.

On a more specific note Searing presents an empirical analysis showing that electoral margin has no significant effect on an indicator of constituency effort, "time spent in constituency," contained in a data set from his earlier research. He generously suggests that our data may be better, or that real change occurred between the early and late 1970s. Regarding the first possibility, we have no precisely comparable variable in our data. The closest we can come is "time spent on casework." Like Searing's variable this measure of time commitment is not closely related to electoral margins. It bears a negative but insignificant relation to previous (1974) electoral margin, and a positive sometimes weakly significant ($p < .10$) relation to succeeding (1979) margin, suggesting that a close election stimulates constituency work which in turn produces the desired electoral impact. But the relationships are weak and not robust. On the other hand, as shown in our forthcoming book, caseloads, number of surgeries, frequency of visits to the constituency and publicization of casework are indicators that relate more consistently to electoral conditions.

We have some questions of our own for Searing's analysis. He places great store in "role" ("those for whom constituency service is the principal career interest") as an explanation for members' constituency service. Like most American legislative scholars we are suspicious of explanations based on roles. Does one really explain "time in constituency" by finding that "those for whom constituency service is the principal career interest" spend more time there? Or has one simply performed a validity check on the concept? Moreover, where do roles come from? Rose (1982) discusses eight roles adopted by contemporary MPs. Why do some adopt the role of "constituency representative" and others not? Might it even be the case that those least electorally secure are more likely to adopt the role of constituency member? We would be curious to know the answer.

In sum, we fully agree with Searing that it is premature to make any claims regarding the principal motivations for constituency service in Great Britain (or even the U.S. for that matter). We make no such claims. As our forthcoming book will show, however, members do believe constituency service has an electoral impact, it does appear to have some impact, and patterns of service are at least consistent with the notion that electoral considerations have some bearing on who does what and how much service.

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Forthcoming Articles

The following articles have been scheduled for publication in the March, 1986 issue:

Robert Axelrod, "Presidential Election Coalitions in 1984"

James E. Campbell, "Presidential Coattails and Midterm Losses in State Legislative Elections"

Arthur T. Denzau and Michael C. Munger, "Legislative Representation of Unorganized Interests"

Judith Goldstein, "The Political Economy of Trade: Institutions of Protection"

Harold K. Jacobson, William M. Reisinger, and Todd Mathers, "National Entanglements in International Governmental Organizations"

Douglas Madsen, "Power Seekers Are Biochemically Different: Further Evidence"

Tevfik F. Nas, Albert C. Price, and Charles T. Weber, "A Policy Oriented Theory of Corruption"

G. Bingham Powell, Jr., "American Voter Turnout in Comparative Perspective"

George Rabinowitz and Stuart Elaine Macdonald, "The Power of the States in U.S. Presidential Elections"

Steven B. Smith, "Hegel's Critique of Liberalism"

Patricia Springborg, "Politics, Primordialism, and Orientalism"

Walter J. Stone, "The Carryover Effect in Presidential Elections"

BOOK REVIEWS

Review Essay

Perspective on Ratzel's Political Geography. By James M. Hunter. (Lanham, Md.: University Press of America, 1983. Pp. xxi + 544. $33.50, cloth; $20.75, paper.)

Pluralism and Political Geography: People, Territory and State. Edited by Nurit Kliot and Stanley Waterman. (New York: St. Martin's Press, London, Croom Helm, 1983. Pp. viii + 323. $30.00.)

The Fragmented State: The Political Geography of Power. By Ronan Paddison. (New York: St. Martin's Press, 1983. Pp. xi + 315. $25.00.)

The term political geography was coined in 1751 by Anne-Robert-Jacques Turgot. His *Plan d'un ouvrage sur la geographie politique* is however still essentially in the same vein as the writings of Hippocrates, Aristotle, or Montesquieu which focused primarily on an alleged *direct* impact of climate and physical geography on man and particularly on human nature.

It is Friedrich Ratzel (1844-1904) who is rightfully considered the father of this branch of geography. Although not free of traces of a simplistic geographical determinism, it is his theoretical *magnum opus, Politische Geographie* (1897; 2nd ed. 1903) that ushers us into the age of modern political geography. Of interest to Americans is also the fact that he is the author of the first political geography of the United States.

For more than half a century, however, we in America have heard very little about Ratzel, at least very little positive. First it was the American geographers who began, in the 1930s, to distance themselves from the whole Ratzelian anthropogeography that emphasized the role of the man-earth relationship in general and also in political geography. They wanted (and ultimately succeeded) to transform geography into a social science primarily, and for this reason felt they must struggle hard against the "influences of geographical environment" school of thought spread in America by a devoted follower of Ratzel, Ellen Churchill Semple. Then came World War II and a concerted criticism, justified but exaggerated, of the Karl Hauhofer school of geopolitics, and because Haushofer considered Ratzel to be the grandfather of his thoughts (the direct inspiration of which he ascribed to Rudolf Kjellen as well as to Halford Mackinder), the axe fell on Ratzel, too. Last but not least, the post-World War II American intellectual scene became dominated by an ideology proclaiming the complete freedom of the individual and of his will. Humans are agents free from determination by heredity, physical or social environment, and cultural heritage. For the same reasons Margaret Mead's anthropological studies (now discovered to be wanting) were then greeted with enthusiasm, a new, more thorough purge of all "geographic influences" was undertaken. If America was a "God blessed country," it was not (as previously believed) primarily because the Creator has endowed it with an unusually large share of fruitful land and other bounties of nature and climate, but because the Americans have of their free will chosen the most efficient and overall happiest social and political system. If only the other countries would adopt a similar system they, too, would become as rich and happy.

Then however came the environmentalist movement that pointed out that not everything continues to bloom in the American Garden of Eden. If we want anything to bloom at all in it in the future, we must recognize that the natural environment does impose limits, sometimes quite severe, upon our freedom of action and choice of life styles. This, together with the shaking up by the Vietnam debacle of the American confidence in the unquestionable superiority and universal applicability of its ways, prepared the ground for a receptive acceptance of a critique of past judgments.

Thus, James Hunter's exercise in the rehabilitation of Ratzel, for this is what his book is, comes at a very propitious time. Indeed, it was with amazement that one could hear at the 1984 Paris Congress of the International Geographical Union speaker after speaker competing in an effort to restore to honor the once admired, then condemned, Friedrich Ratzel on the eightieth anniversary of his death; and rehabilitated with him were such other "heretics" of the environmentalist, and even deterministic, geographic school of thought as Griffith Taylor and Ellsworth Huntington. The arguments ran from "they were not really environmentalists," or "only mildly deterministic," to the admission that perhaps a certain dose of geographic determinism is what realism and scientific truthfulness requires us to acknowledge in discussing historical developments, even those in industrialized countries.

The thrust of *Perspectives on Ratzel's Political Geography* is a (defensive) explanation of certain Ratzelian concepts and "laws" of development and, above all, his exculpation from the charge that he took his organismic theories literally and preached geographic determinism. In his unabashed admiration for Ratzel—no sin in itself—Hunter parallels that of W. H. Parker for Halford J. Mackinder, the author of the famous paper on the world geopolitical Heartland written in the year Ratzel died and decisively contributing to the further development, and popularization, of the fields of political geography and geopolitics all through the twentieth century. However, although Parker's biography may be reproached as too uncritical of Mackinder but is otherwise praiseworthy from a scholarly and literary point of view, Hunter's work is on these last two points grievously wanting.

An unusually large portion of Hunter's book consists of long German quotations from Ratzel's works which are then translated by the author. However, these translations are most of the time literally atrocious, sometimes just in terms of the English language but more often also inaccurate, misleading, or even outright wrong in terms of the original meaning. Briefly, whoever cannot read the original German quotations should be warned from relying on Hunter's translations, and whoever can read German is justified in not trusting Hunter's exposition of Ratzel's ideas, because very few of his writings have been translated. Certainly, Ratzel merited a better defense than this.

By contrast, in *The Fragmented State* Paddison offers us an excellent example of what political geographers do today. Like many of his colleagues, especially in the United Kingdom, he has a deep interest in the internal political geography of the state, focusing on the communal, urban, and regional political entities. In this study he turns his attention to the various centers of power within the state; how is power distributed among them, how and why it shifts from one center and level to another, and what are the political and spatial consequences of such a decentralization, and fragmentation, of political power. For illustration, and comparative purposes, he takes his examples from several countries, but limits his sample to developed Western states.

Some political scientists may ask, But is it geography? Well, yes, it is, provided we are aware contemporary geography is (at least in the West) a social science focusing on the dynamics of the ever changing pattern of distribution of phenomena in space. In other words, the "new geography" is far less likely to pay primary attention to various permanent spatial phenomena, such as already well-known, explored, and mapped features of the natural landscape, because, being a social science, it is much more interested in those phenomena and patterns that are ever evolving as a result of human decisions and activity as well as changes within the society itself. Hence, political geographers tend to focus on the spatial consequences of politics or on the political causes of the dynamics of spatial patterns, for instance, political power, the state's territory, the center-periphery relationship, or electoral geography.

Pluralism and Political Geography edited by Kliot and Waterman is a perfect sampler of short but in general solid contemporary political geographical writing. Authors from several countries represent a variety of trends and interests, from theoretical to applied and from macro to micro territorial, with a focus on intrastate as well as interstate politics, and a discussion of such topics as boundaries, frontiers, pluralism, and minority problems. One writer even gives us a full-blown prescription for how America should conduct its foreign policy in the 1980s. Prescriptive political geography or geopolitics is, of course, perfectly legitimate, but for an essay legitimately to claim a place in a volume of political geographical writings, it should contribute at least *some* ideas, perspectives, or insights that are indeed recognizably political geographical or geopolitical.

For a political scientist not well versed in the controversies among political geographers as to what their field is, or ought to be, the essays in the Part 1 of the volume and the essays by Paul Claval and Shalom Reichman should be most useful. No less valuable to many a political scientist should prove to be the three essays that discuss problems of frontiers and boundaries, but practically every political scientist, even one not especially attracted by political geography, is likely to find here something of interest. Indeed, this is a volume well suited for undermining the indifference that many political scientists unfortunately display towards a field that could add a new and vital dimension to their own domain of study.

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American Government and Politics

The Politics of Unemployment. By Donald C. Baumer and Carl E. Van Horn. (Washington, D.C.: Congressional Quarterly Press, 1985. Pp. xiii + 224. $8.95, paper.)

This work examines the ways the federal government responds to unemployment and, more specifically, it describes the development of "government programs designed to help the unemployed either by treating the symptoms of unemployment or by providing jobs or training" (p. 6). The authors do note that their focus is "on governmental spending programs that attack unemployment directly, rather than on broader policies and strategies that indirectly affect the overall level of unemployment in society" (p. 7) as the title might suggest.

After describing, in chapter 1, the major federal unemployment programs, Baumer and Van Horn examine the differences in how the policy process deals with unemployment programs and the way it deals with other policy issues. The major organization scheme of this book is the "four seasons of unemployment policy" presented in some detail in chapters 3 through 6. Briefly, chapter 3 analyzes the first season, from 1973 to 1976, focusing on the enactment and early implementation of the Comprehensive Employment and Training Act (CETA). Chapter 4 covers the second season, 1976 to 1978, examining the expanding job programs and experimental youth employment programs. Chapter 5 describes efforts made by federal policymakers to write more specific statutes between 1978 and 1980, the third season; chapter 6 describes the fourth season, 1980 to 1984, during which the Reagan administration introduced new job programs. Throughout this volume the relationship between policymaking and policy implementation is stressed, emphasizing the dynamics of competing interests, intergovernmental coordination, and alternative enforcement strategies that comprise policy implementation.

Although this work presents a brief yet comprehensive review of unemployment programs from 1932 to 1984, the general reader will perhaps be most interested in the evolution of CETA. CETA started in 1973 as a relatively small, decentralized job training program and by 1980 grew into a multifaceted, balkanized, greatly expanded program that was ultimately replaced by the Job Training Partnership Act (JTPA) signed by President Reagan in October, 1982. Between 1978 and 1982, CETA was a large fragmented system consisting of four elements: training and remedial education, private-sector initiative programs, youth programs, and public service employment. Each program had been established by special legislation resulting in separate operations, duplication of services, confusion, and fragmentation of the delivery system (p. 139). Partially in response to these implementation hurdles and partially a reflection of the existing political climate, CETA underwent a major revision in 1982.

Baumer and Van Horn present an excellent political analysis of the forces and events shaping CETA's evolution into JPTA. In addition to reviewing the influence of subgovernmental agencies in the pro-CETA and anti-CETA policy networks, the authors discuss the role of policy research in the policy process. Although acknowledging that "research is used by advocates to back up their own points of view," Baumer and Van Horn observe (p. 175):

> Even though research findings alone did not determine the shape of the new law, the thorough body of evidence suggesting that CETA programs had worked was used effectively by CETA's defenders. Had this evidence not existed, it would have been difficult, if not impossible, to build support for continuing federal job training programs.

This volume is a very readable work that is a good blend of political and policy analysis. It comprehensively presents a great deal of facts about federal unemployment policy. Two omissions weaken the generally excellent quality of this analysis, however. First, the volume would be improved with more discussion of the evaluation of the actual impacts of unemployment programs. For example, the Mathematica Policy Research Inc. evaluation of the cost effectiveness of the Job Corps has been a key factor in congressional discussions of its continuation. This study is often cited as an excellent example of policy research. Second, although the focus on unemployment programs is certainly defensible, and the authors briefly mention some macroeconomic issues such as cyclical unemployment and the effects of declining productivity, this narrower focus makes it difficult to assess the overall effectiveness and efficiency of unemployment programs relative to more macro economic policies. This latter omission is especially important during the Reagan administration, the priorities of which are away from social programs to mitigate the effects of unemployment and presumably towards policies designed to stimulate employment.

In addition to furthering our knowledge of unemployment policies and the links between agenda-setting, policy adoption, and policy implementation, *The Politics of Unemployment* should serve as an excellent example of mid-range policy analysis. Baumer and Van Horn provide a sound analysis of not only what policy tells us about politics, but also how policy affects the politics of implementation and policy reconsideration.

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Political Action: The Key to Understanding Politics. By George Beam and Dick Simpson. (Athens, Ohio: Swallow Press, 1984. Pp. ix + 253. $19.95, cloth; $9.95, paper.)

Beam and Simpson have written *Political Action: The Key to Understanding Politics* out of their conviction that modern political science is bankrupt because it "can neither specify the proper institutions for a humane society nor guide effective political actions to obtain our goals" (p. 4). Uncomfortable with a political science based on the paradigms of positivism and post-positivism, the authors would substitute a model in which "political understanding is created by bringing together knowledge acquired through analysis, goals acquired through philosophy, and strategy acquired through the study of political action" (p. 41).

Central to Beam and Simpson's methodology is the development of what they call "political action propositions" in which goals are defined by normative principles, conditions are determined by analysis and the testing of hypotheses, and political actions are designed to achieve the stated goals under the conditions determined by the analysis. The general proposition is directed toward building institutions for fundamental change. The goals are more freedom and citizen participation; the conditions a liberal-democratic government, a postindustrial economy, and a larger professional class; and the action the creation of new political institutions. The proposition has three stages, each with various requirements: getting started, mobilizing and applying resources, and institutionalizing change (p. 59).

Much of the general action proposition reflects common sense and common experience. For example, the authors state that getting institutions started requires such things as choosing the right time, acquiring effective leaders, and winning early victories. And mobilizing resources requires, among others, involving people not now participating as well as mobilizing the professional class and developing a full-time staff. (A certain note of elitism appears regularly in this work and is never more apparent than in the authors' insistence that professionals with their positions, skills, and time are absolutely necessary for success.)

The authors find philosophical roots for their action proposition methodology in what they call the "pragmatic action" philosophies of Machiavelli and Alinsky, the "conditional action" philosophies of Marx and Marcuse, and the "moral action" philosophies of Jacques Maritain and Martin Luther King, Jr., although Beam and Simpson find each of these philosophies lacking in its ability to link together the three components (goals, conditions, and actions) of political understanding (p. 80).

Beam and Simpson test their Proposition for Political Change by applying it in three case studies, one of the creation of the Independent Precinct Organization in Chicago, the second of the Civil Rights Movement and the third, a "future case study," an administrative movement to control bureaucracy. Although the model has considerable explanatory appeal in the first two cases, the third case is shaky indeed. In the first place, one must question whether one can conduct a case study of something that hasn't happened. But even if the reader is willing to ignore this quibble, he/she is asked to accept the realistic possibility of an administrative movement in which middle-level administrators, scientists, engineers, and outsiders join together in bringing bureaucratic power under control in both the public and private sectors. It is extremely difficult to imagine what issue might kindle such a movement or further to imagine the requisite leader emerging from middle-level professional administrators. Simpson and Beam complete their call to reform for political science by arguing that as perceptions of the nature of reality have been shifting in modern physics and mathematics through the study of quantum mechanics, in psychology through the theories of Jung, and in theology through the development of creation-centered theology, so they must change in political science through an emphasis on political action and the political action propositions which the authors have developed.

Political Action is a thought-provoking and interesting work. But it falls far short of achieving its admittedly bold purpose in that it hardly seems a likely model for the restructuring of the entire discipline of political science.

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Immigration Policy and the American Labor Force. By Vernon M. Briggs, Jr. (Baltimore: Johns Hopkins University Press, 1984. Pp. xv + 294. $26.50.)

Labor economist Vernon Briggs has written an at times iconoclastic volume on U.S. immigration policy. Although the work occasionally travels a road well-travelled by others, its thesis is unusual in that Briggs does not stop with an analysis of the effects of immigration to the United States on the U.S. labor market during the past 100 years. Instead, he focuses on a little-discussed aspect of immigration policy: the *presumed* failure of immigration policymakers during the last 20 years to give adequate consideration to the labor market needs of the United States or to consider the long-term labor market implications of their policy. Although this may be an important premise, it might not be an accurate enough one on which to anchor an entire volume unless one ignores the infinitely flexible immigration backdoors to the United States: the various de jure and de facto temporary immigration programs and the vast and well-entrenched institution of illegal immigration. These "programs" have been deliberately allowed to prosper precisely in order to make immigration more consonant with the fluctuating needs of the nation's various regional labor markets.

Hence the volume's weakness: an often distracting tendency to assign responsibility for this failure to a number of groups rather than processes—the lawyers in control of immigration policymaking, the ethnic and religious groups who often become veto groups in the various phases of the immigration debates, the policymakers who "tinker" with immigration legislation "for often dubious political purposes . . ." (p. xii). Although these are indeed important issues, and Briggs is fundamentally on target in many of his criticisms, his prescription to make immigration policy more sensitive to the labor market requirements of the United States within the realities of the U.S. political process is never fully developed.

As a result, the strength of *Immigration Policy and the American Labor Force* shifts away from Briggs's stated thesis to its other rather formidable assets: the depth of its historical analysis; the wealth of information on the evolution of the American labor market—and the place of immigration in that evolution; the excellent discussions of such issues as this nation's nonimmigrant labor policy and the U.S.-Mexican border labor market; the informed and balanced discussion of the issue of illegal immigration; and the valuable and sensitive analysis of the nuances in the ongoing policy debate about immigration reform.

These contributions alone make the work a valuable addition to a still embryonic field of study where analysts continue to speak different languages and to suffer from a corresponding lack of cross-disciplinary learning. Yet Briggs's disclaimers and explanations notwithstanding, one must question the wisdom of some of the choices evident throughout this work: the decision to eschew adopting a theoretical framework, or at least offering a discussion of the various theories that would certainly have enhanced the mining potential of the work; the failure to consult some of the rich sources on the social and political context of the debate on U.S. immigration from 1860 to 1930 (the absence of John Higham's works are notable in this regard, especially his *Strangers in the Land: Patterns of American Nativism* (Rutgers University Press, 1955); and, in an uncharacteristic research lapse, the tendency to equate illegal immigration with Mexican illegal immigration in spite of the accumulating evidence that Mexicans account for only about 50% of illegal immigrants.

Vernon Briggs has written a book with a point of view. *Immigration Policy and the American Labor Force* is a powerful, impassioned, at times frustrating, but always informative book. Everyone with an interest in the topic will benefit from reading it, not only because of its facts but also because it forces one to rethink the principal focus of U.S. immigration policy since 1965: the principle of family reunification. That this single emphasis should be debated—Briggs's repeated plea—deserves a hearing. That it will give rise to a policy and political maelstrom might be a legitimate concern to those interested in a thorough, but not paroxysmal, reevaluation of this nation's immigration policy.

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The Aggressions of Civilization: Federal Indian Policy since the 1880's. Edited by Sandra L. Cadwalader and Vine Deloria, Jr. (Philadelphia: Temple University Press, 1984. Pp. v + 258. $34.95.)

The title of this book of readings comes from a nineteenth-century statement by the Indian Rights

Association, a benevolent organization founded by white advocates of the philosophy of cultural assimilation. The "aggressions of civilization" discussed are primarily those conducted under the auspices of the democratic institutions and the legal system of the United States.

The unique and contradictory political and legal status of American Indians is a central concern of this book. The evolution of public policy towards Indians is well documented in terms of its roots in the prevailing ideological currents of the white majority. What has remained a constant is the survival of tribal entities amidst swings of the legislative pendulum from determined efforts to dismantle the tribal land base and undermine tribal authority to reinforcement of self-determination.

No issue has plagued Indian-white relations as greatly as territorial or property rights. Aboriginal land tenancy was recognized by the earliest European adventurers and Indian legal rights have largely been derived from treaties involving land transactions. Treaties are recognized in the Constitution as constituting part of "the Supreme Law of the Land" and promises of "utmost good faith" to respect Indian land were included in the Northwest Ordinance of 1789. However, as two of the authors demonstrate, despite the appearance of rigorous legal protection, "United States law and the courts have substantially failed to protect Indian lands from arbitrary confiscations, partitioning, bureaucratic control, and treaty violations" (p. 187). Perhaps the best known of these transgressions was the passage of the General Allotment Act of 1887, which provided for the dissolution of tribal holdings through the allotment of land into individual ownerships. This redistribution of land from communal to private holdings was viewed as the key to cultural assimilation and used as the basis for eventual citizenship. As Vine Deloria, Jr. points out, congressional efforts of this era provided individual rights for Indians apart from their tribal identity in that they "centered on the distribution of property rather than on the articulation of traditional liberties" (p. 116).

The Indian Reorganization Act of 1934 is generally viewed as a turning point away from the idea of assimilation to the recognition of tribal self-government. It followed, by a century, the Marshall Court definition of the status of tribes as that of "domestic dependent nations" wherein political authority within Indian country resided with the tribal unit, but that tribal entity was limited by the supremacy of the federal government. As conflicts have arisen over the limits of authority of the respective governing bodies, virtually every facet of Indian life has been affected. A proliferation of legislation and court rulings during the last 50 years have further delineated federal-Indian relations, but there remain many unresolved issues.

This is a well documented yet concise book that summarizes effectively the morass of federal policy dealing with Indian affairs and provides a useful introduction to one of the most complex histories of human relationships.

KATHLEEN O'BRIEN JACKSON

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Party Organizations in American Politics. By Cornelius P. Cotter et al. (New York: Praeger Publishers, 1984. Pp. xii + 203. $31.95.)

This book fills what was a substantial gap in the literature on parties. Previous studies of party decline have assumed that a relationship exists between the party in the electorate and party organizations. But this work clearly shows that declining party identification in the 1960s and 1970s was accompanied by parties with stronger organizational capacities. This is an important finding.

The book would have benefited from a more careful presentation of the data. For example, none of the regression coefficients in chapters 4 or 6 are accompanied by standard errors (or evaluations of statistical significance). This is particularly troublesome in chapter 6, where a number of the coefficients are very close to zero and cause one to question the curvelinear shape of the relationship the authors claim to have found (e.g., Figures 6.3 and 6.7 on pages 119 and 124).

There are also a number of tables where differences between Democratic and Republican patterns are indicated, such as Tables 6.1 and 6.2 on pages 112 and 115. Simple tests of the probability of the observed differences being due to chance would strengthen the findings. It is true that these are not random samples, but one can still test the likelihood of achieving such results by chance.

In a similar fashion, the hypothesis that greater interparty competition is related to party organizational strength would be tested more directly by a simple correlation coefficient than by the difference of means by category of competition found in Table 5.2 (p. 89), whereas the impact of organizational strength on electoral success would be better measured by simple bivariate regression than by the correlations found in Table 5.5 (p. 101).

Finally, the authors seem to misinterpret Table 6.4 (p. 125). They state that stronger state-local party integration leads to more successful linkage between the party and legislative leadership for the Democrats because the former variable accounts for a large amount of the variance of the latter variable. (Table 6.4 gives only the R^2 figures.) But if Figure 6.7 is to be believed, "increments in party integration are generally associated with decrements in party-legislative relations" (p. 124). Thus, the stronger R^2 results from an inverse, not a positive relationship. It may be that the multivariate analysis of Table 6.4 reverses the findings of Figure 6.7, but no figures to support such a conclusion are given.

The authors nevertheless develop useful measures of party strength and document the continuing vitality of parties as they attempt to adapt to a changing political environment. The examination of the roles of parties at both the state and local levels and the effect of organizational strength on other party activities such as elections and linkage is enlightening and, as far as I know, unique. Among other things, the authors find that parties have increased the scope of their activities in the last 20 years, that public policy has not damaged and may actually have helped party organizations, and that the strength of state party organizations makes a difference in gubernatorial elections.

In the final chapter, the authors discuss the revisions of our theories of parties that their results may infer. This discussion is brief but provocative, asserting that parties may actually create a "counter-realigning" or a "counter-dealigning" effect. I might have preferred a more extensive discussion of these issues, but that is asking a great deal. As it now stands, this is an important book. No theory of American political parties or party decline can afford to ignore the evidence presented here. And I think the authors' findings will force us to reevaluate our views about the future role of parties in the American political universe.

ARTHUR SANDERS

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Nazis in Skokie: Freedom, Community, and the First Amendment. By Donald A. Downs. (Notre Dame, Ind.: University of Notre Dame Press, 1985. Pp. xii + 227. $20.00.)

Civil Liberties and Nazis: The Skokie Free-Speech Controversy. By James L. Gibson and Richard D. Bingham. (New York: Praeger Publishers, 1985. Pp. xi + 227. $34.95.)

These two volumes are fundamentally different both in scholarly focus and in political stance. The Gibson-Bingham book analyzes the reactions of American Civil Liberties Union (ACLU) members and Common Cause members to the controversy generated by the ACLU's representation of National Socialist Party of America (NSPA) leader Frank Collin in his 1977 effort to picket the village hall of Skokie, Illinois, a Chicago suburb whose population included hundreds of Jewish survivors of the German Nazi Holocaust. While Gibson and Bingham openly endorse the ACLU's stance and argue that Skokie produced only minimal defections from the ACLU's ranks, Donald Downs asserts that the threatened Nazi demonstration created "substantial harms, such as emotional trauma, the breakdown of civility, and the threat of massive violence" (p. 120) in Skokie, injuries that more than justify the complete removal of First Amendment protection from "targeted racial vilification" (p. 138) as practiced by Nazis.

Gibson and Bingham are interested less in the Skokie story than in how reactions by members of the American "elite" to the First Amendment controversy allow for improved analysis of the concept of political tolerance in public opinion research. They note that "Americans demonstrate a strong tendency toward intolerance of political non-conformists" (p. 14), but that much research has shown elites to be more tolerant of dissent than the mass public, and posit that ACLU and Common Cause members come from America's elite. Hence it is with some chagrin that Gibson and Bingham report their sample survey results that show that a substantial minority of ACLU members (though not ACLU *leaders*), and at times a majority of Common Cause members, adopt intolerant, anti-free speech positions. Acknowledging that their results "are not very supportive of the presumption that elites are consensually and strongly supportive of the rights of political opposition" (p. 68), Gibson and Bingham go on to suggest that "the general elite consensus on democratic values is illusory" and perhaps irrelevant, if the judiciary, and some members of the legal elite are sufficiently committed to protecting minority rights (p. 191).

In a different context, Gibson and Bingham also argue that political tolerance is not unidimensional, as much past research has assumed, but "a *syndrome* of connected beliefs" which is multidimensional and constrained (pp. 133-134). Similarly, Gibson and Bingham also hope to correct what they regard as a basic error in previous research that has focused on the "slippage" between higher levels of tolerance in abstract situations and lower levels for specific examples drawn from the same general principles but involving political dissenters. "The 'abstract-concrete' problem . . . has been misconstrued as a matter of logical consistency," they contend; instead, "opinion formation represents a process of adjudicating conflict among conflicting general attitudes far more than it represents the deductive application of principles to specific cases" (p. 184).

Describing their respondents, Gibson and Bingham note that "most of those supporting the Skokie position searched, sometimes quite creatively, for a constitutional theory compatible with their preferred outcome. Most individuals can identify, or suggest, a rational-legal basis for their intolerant positions" (pp. 80-81). That description also can apply to Donald Downs's solid but ultimately flawed constitutional analysis of the Skokie controversy. Downs wants to revive and expand the *Chaplinsky* v. *New Hampshire* (1942) doctrine that "fighting words" are not protected speech under the First Amendment; as Downs reminds us, *Chaplinsky* "is consequentialist in nature: the First Amendment protects speech which contributes to 'the exposition of ideas' and 'social values'" (p. 84), and only that. Downs unsuccessfully tries to deny that he is advocating simply a utilitarian weighing or balancing of interests as a First Amendment standard; in introducing his major chapter he flat-footedly indicates that his purpose is "to show why the harms of the NSPA speech threat outweighed the benefits" (p. 122).

To Downs the anti-speech interests were constitutionally weightier in Skokie because of the "emotional trauma" that the Nazis' rhetoric, regalia, and desire to picket inflicted upon the village's Holocaust survivors. "Targeted racial vilification," which Downs defines as "derogatory reference to race directed at a predetermined target for the purpose of intimidation" (pp. 131-132), ought to be constitutionally unprotected because it is "inherently traumatic and assaultive" (p. 138). Such a formulation makes determination of the protesters' purpose or intent central to the constitutionality test, and Downs believes such a judgment can and should be made. Expression whose "primary purpose" is communication ought to be protected, while that which intends "the infliction of harm" ought not to be: "There is a difference between a speech act that appeals to reason and the conscience and one which is primarily assaultive" (pp. 127-128).

The wide-ranging vagueness of Downs's approach is magnified even further by his explanations of when expression is "traumatic" or "assaultive" in impact and hence unprotected. In Skokie the Nazis' potential appearance was traumatic because it would "inflict a sense of insecurity and incivility," leaving the Holocaust survivors "terrorized" by the "triggering of painful memories" (pp. 91, 123, 93). Likewise, Downs holds that "expression is assaultive when it seriously injures the dignity or self-esteem of a person" (p. 155).

Under content-oriented standards such as those, judges who share Downs's sympathies for hate-group targets could hold much unpopular speech to be constitutionally unprotected. Downs would further maximize that danger by two additional antiprotection innovations. First, "targeted expression may be harmful even when it is not explicit," or even *verbal,* he asserts, and hence "targeted symbolic expression [e.g., a swastika or Nazi uniforms] should be abridgeable even though it is not explicitly villifying" (p. 158). Second, Downs contends that "assaultive expression should be abridgeable despite the fact that it may be 'mixed' with worthy or even truthful speech" (p. 164).

Downs indicates that he was deeply touched emotionally by his personal interviews with Holocaust survivors in Skokie, and that personal sympathy has seriously distorted his analytical rigor. Describing Nazi leader Collin, Downs explains that "an extremist is somebody whose political values, goals, and strategies are a function of his emotional needs rather than a function of his reason," as impliedly is the case with nonextremists (p. 23). Downs quotes his Skokie interviewees as terming the ACLU's position as "beyond belief" (p. 52), and the ACLU attorney as "a rotten person and an opportunist . . . scum" (p. 106). Downs acknowledges those comments are "unrealistic and extreme" (p. 109) and seeks to defend their vehemence ("she was emotional, and at times 'prejudiced' . . . but always with intelligence and control," p. 41), but he fails to realize how such frank quotations embarrassingly contradict his dismissively simpleminded definition of extremism. Although Downs's headstrong sympathy for the survivors' feelings and preferences may be understandable and personally appealing, it has resulted in a provocative but badly flawed piece of constitutional analysis.

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Conservatives in Court. By Lee Epstein. (Knoxville: University of Tennessee Press, 1985. Pp. xii + 204. $17.95.)

The use of the courts by interest groups to achieve political ends is not a new topic for political science. The literature on this topic has been limited, however, and has tended to concentrate primarily upon the tactics and strategies of liberal interests such as the NAACP and ACLU. *Conservatives in Court* is a systemic appraisal of the participation of conservative interest groups in the judicial arena. In this connection, Epstein's book fills a gap that exists in the literature and represents a significant contribution to our understanding of judicial politics.

The first chapter is a well-written review of the literature about interest group litigation. This review identifies four assumptions about interest group judicial activity. These assumptions are:

> 1. Interest groups often resort to litigation when they view their goals as unobtainable or difficult to achieve in other political forums.
> 2. Sponsorship is the preferred strategy of interest group litigators.
> 3. The resources interest groups need are money, government support, frequent and increasing use of the courts, expertise, extra-legal publicity, and intergroup support.
> 4. Interest groups generally obtain their objectives through litigation. (p. 15)

Epstein suggests that these assumptions have been developed by looking principally at liberal interests, and that they may or may not apply when discussing conservative interest litigation. The body of the book examines these assumptions in the context of conservative economic, social, and public interest litigation.

In the chapter on conservative economic litigation Epstein examines in detail the activity of three groups: The National Right to Work Legal Defense Foundation, the National Chamber Litigation Center, and the Equal Employment Advisory Council. In general, the assumptions cited above are all demonstrated to be inaccurate to varying degrees for conservative economic groups.

Similar results are presented in the chapter on conservative social litigation. Here Epstein analyzes the activities of three groups: Citizens for Decency through Law, Americans for Effective Law Enforcement, and Americans United for Life Legal Defense Fund. The chapter evaluates the attempts of these groups to represent conservative positions on sexually explicit material, criminal procedure, and abortion respectively.

Conservative public interest litigation is also carefully examined; Epstein traces the developments associated with the National Legal Center for the Public Interest and the Washington Legal Foundation. Each of these groups has had an important political impact that can be expected to grow should the Supreme Court continue along the conservative path that it has developed under the Chief Justice Burger and the conservatism that may accelerate with potentially three more Reagan appointees. Again, the assumptions derived from the study of liberal interest litigation do not hold true for these conservative groups.

One of the most consistent findings presented in *Conservatives in Court* involves the assumption that interest groups utilize the judicial arena because they do not believe they can achieve their goals in other political forums. Epstein demonstrates that this assumption is untrue for all of the conservative groups examined. In fact, these groups have been quite successful in the legislative arena, but felt that they were being underrepresented in the judicial forum.

A very interesting observation is presented concerning the funding of conservative interest litigation. It appears that money for these causes is tied to two major sources: direct mail fund raiser Richard Viguerie or the Scaife Family Foundation. This observation could be the basis for interesting research into the breadth of support necessary for conservatives to succeed in the judicial arena.

The judicial activity of conservative groups is thoroughly documented and interestingly described by the author. Overall, *Conservatives in Court* is a well-written and informative book that adds significantly to our knowledge of interest articulation in American society. This book should serve as a springboard for further investigation into the politics of the judicial forum.

ALBERT C. PRICE

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Presidents and Promises: From Campaign Pledge to Presidential Performance. By Jeff Fishel. (Washington, D.C.: Congressional Quarterly Press, 1985. Pp. xv + 226. $9.95, paper.)

The importance of the connection between what presidential candidates promise and what they actually do after entering office is as important as democratic politics itself. Yet systematic study of this question is rare. Fortunately, Fishel's study of presidential promises and performance is an incisive, insightful, and detailed examination of platform-performance consistency for domestic proposals from the Kennedy through Reagan administrations (and written with pleasingly wry winks to the reader).

Fishel relies on three data sources: interviews with 136 present and former administration officials from the last six presidencies; content analysis of presidential documents including speeches, platforms, position papers, and legislative proposals and executive orders; and, various library and archive material from past presidencies and the parties. To the author's credit, he also relies prudently and judiciously on important and pertinent related scholarship.

After a brief overview of the connection between promises and policy in chapter 1, Fishel lays out in his superb chapter 2 the results of his quantitative assessment of promise and performance from Kennedy to the present. He immediately confronts a central problem in agenda formation—that of ambiguity. We immediately observe one of the central paradoxes of agenda development: ambiguity is politically advantageous (because of cognitive processes that encourage voters to see candidates as they wish them to be), yet is inimical to the very idea of voters being offered meaningful, distinguishable choices at the polls. This paradox is examplified nicely by the Carter administration, which made more specific promises than any president examined in the book (pp. 32-34). These promises provided ample ammunition for Carter's critics. Yet at the same time, pollster Pat Caddell was advising Carter both in the campaign and the presidency to "emphasize style over substance" (this recommendation coming from Caddell's 56-page postelection paper to Carter). Ambiguity, then, is politically expedient but intellectually odious. As Carter himself observed after his 1980 defeat, "Campaign promises are poison" (p. 189).

Fishel observes that agenda development is the product of the political wisdom of campaign decision makers and the countervailing pressure of external political groups and forces anxious to elicit firm, specific pledges of support (pp. 21-22). This formula manifests itself in somewhat different ways in each party.

Recognizing the problems in marrying promise to performance, Fishel nevertheless nicely quantifies the two and concludes that presidents since Kennedy have attempted to implement roughly two-thirds of their preelection promises. The only president who bucked this trend was Nixon, who took no action on 27% of his promises. Moreover, the much-maligned Carter administration led all others with the highest ratio of proposals to pledges.

Interestingly, Fishel observes that presidents who attempt to dramatically change policy agenda directions are in fact often subject to political criticism and punishment (pp. 29-30). Thus, given environmental, political, and institutional constraints, presidents as candidates make promises they mean; as incumbents, they do work to implement those pledges. Presidents are serious in their campaign pledges; moreover, "Presidents control what they initiate; other institutions control what presidents achieve" (p. 187).

Fishel's middle chapters focus specifically on Carter's and Reagan's promises (chapters 3 and 5, respectively) and their actual performance (chapters 4 and 6, respectively). These are excellent, detailed summaries of how the Carter and Reagan agendas were shaped, and how they fared.

The concluding chapter emphasizes, for this reader at least, the continuities that continue to shape the course and direction of presidents regardless of party, ideology, mandate, or even skill. In particular, Fishel observes the nearsighted nature of agenda formation. When the primary question facing candidates and incumbents is "What have you done for me lately?" (or, as Fishel says, presidents' "'long-run' futures are 30 minutes from the present" [p. 214]), is it any wonder that presidential aspirants/incumbents fall short of the high standards set for them? Given Fishel's assessment, it seems clear that all presidents deserve more credit for attempting to achieve policy consistency than they receive.

ROBERT J. SPITZER

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Religion and Politics in America. By Robert Booth Fowler. (Metuchen, N.J.: Scarecrow Press, 1985. Pp. xi + 351. $25.00.)

Recent concern about the political activity of conservative Christians has renewed our interest in the linkage between religion and politics. Fowler's work deliberately goes beyond a narrow focus on the New Christian Right to examine whether religion matters in American politics. His answer is mixed. He argues that religion is extremely involved in politics, but he concludes that it has very little influence because of the extreme pluralism that diffuses its impact. This pluralism reflects a Madisonian balance of factions that creates plenty of heat but not much movement.

Fowler focuses on the views of four major groups: Roman Catholics, Jews, liberal Protestants, and evangelicals-fundamentalists. A closing chapter on the black church widens the scope a bit, but the perspectives of this group are not well integrated into the rest of the book. Separate chapters examine the impact of religion on the political culture, attitudes toward political involvement, voting, the White House, and Capitol Hill. There are also two chapters discussing religious interest groups and three examining various theories of church-state relations and several Supreme Court decisions.

One major strength of the book is the central theme of religious pluralism, which suggests that some critics of the conservative Christian groups are exaggerating the fear of a new religious establishment. Related to this, the reminder that there is a difference between involvement and influence is most helpful. The study is well-grounded in the literature; notable in this respect is a 20-page bibliography. Fowler is at his best when he analyzes and critiques major themes in the literature. For example, his argument that religion has often served as a refuge from liberalism rather than a support for it is a thought-provoking challenge to the civil religion thesis. Finally, Fowler adds to interest group theory by suggesting the crucial role of theological predispositions toward change and toward government.

Fowler does not introduce much new information, although the book is a good summary of what is available. The descriptions of various religious groups and Supreme Court cases are not particularly revealing because these stories have been told before. As with many recent studies of the New Christian Right, Fowler fails to distinguish clearly enough between evangelicals and fundamentalists. At two points he discusses some theological differences, but in the rest of the book he treats the two groups as one bloc. Concerning the fundamentalists, Fowler ignores the important intellectual role of Francis Schaeffer. The conclusion that influence comes from organization could be sharpened by realizing that certain types of organization would be better than others—something hinted at in the case studies but not fully developed. In addition, there are a few minor problems: The editing is poor, for example, chapter 7 has two footnotes numbered 1; the section on Capitol Hill could be strengthened by examining the growing network of prayer groups; and the chapter on voting behavior omits the period from 1900 to 1924.

Overall, this study, with its emphasis on religious diversity, is a useful examination of the tie between religion and politics. Much could be added from the 1984 elections, but Fowler's work provides a good foundation for additional research.

ROBERT ZWIER

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CETA: Politics and Policy, 1973-1982. By Grace A. Franklin and Randall B. Ripley. (Knoxville: University of Tennessee Press, 1984. Pp. xviii + 249. $24.95.)

Grace Franklin and Randall Ripley longitudinally examine the training and employability development and the public service employment components of the Comprehensive Employment and Training Act (CETA) from the inception of the act in 1973 to its termination in 1983. The authors examine these selected components of CETA in relationship to the major public policy that preceded CETA (the Manpower Development and Training Act of 1962) and that succeeded CETA (the Job Training Partnership Act of 1984), the four other major block grant programs that emerged with CETA between 1966 and 1975, and within the context of general knowledge about American politics and public policy. This approach's purpose is to determine the extent to which Franklin and Ripley's research findings about the programs support or depart from generally known patterns of American politics and public policy. The book's central objective is to analyze relationships among variables in order to explain variations in the implementation of the training and employability development and the public service employment components of CETA at the local government level.

Franklin and Ripley combine a generous use of secondary analysis of a vast empirical literature on CETA with a substantial amount of original research to assemble a massive data base for their research. The original data base for the book consists of several studies conducted by the authors and funded by the Department of Labor (DOL). The empirical data were generated by thousands of interviews with knowledgeable people in Washington (both officials of DOL and representatives of interest groups active in the manpower and employment issue area), five of the 10 regional offices in DOL, and 33 different localities (with three of those localities being state operations).

Most important among the authors' findings are the facts that most of the changes in policies and in program details were attributable to political factors, and many of these political factors inhibited maximum programmatic service to the most disadvantaged portion of the eligible population. Franklin and Ripley also find that, within the context of these political factors, the professional employment and training staff exerted the most consistent influence. Other findings include the facts that the role of the private sector in the selected CETA programs was limited; advisory councils had only marginal impact on policy outcomes achieved at the pleasure of the local staff and public officials; and the Department of Labor ended up recentralizing and recategorizing CETA programs to the contradiction of the initial legislation of 1975 and, according to the authors, to the detriment of effective program performance. Finally, Franklin and Ripley conclude that all of the problems notwithstanding, the training and employability development component of CETA resulted in generally productive programs for the participants, but its accomplishments were dwarfed by the much larger public-service employment component whose public perception and presumably performance record were less favorable.

The book has several strengths, the primary one being that its research is based on a longitudinal examination of the two selected programs over the entire life of the policy. That is clearly a rarity in the study of public policy, and the principal benefit is that it provides a basis for the comparative analysis of other major public policy programs. Also, the findings of the study are significant and are well within the allowance of the data consulted.

Unfortunately, the study suffers from several weaknesses, although they tend to be relatively minor and procedural. For instance, in the body of the book, the authors seemingly ignore their pledge in the Preface to focus on an examination of training and employability development while addressing public-service employment only occasionally. Instead, public-service employment receives substantial coverage, and the book is better as a result. The only point here is that the Preface and the body of the book should be consistent. Further, it is regrettable that only the most important statistics used by the authors are included in the book, and the large majority of those are placed in the footnotes or appendixes. Although the authors' wish not to burden the analysis with technical matters of methodology is understandable, perhaps this dilemma could have been more appropriately solved by a methodology chapter with an admonition in the first chapter that readers not interested in an in-depth discussion of methodology should skip the designated chapter.

Another weakness of the book is that the analysis chapters (2-5) do not flow well. The authors admit this on page 28, when they write "much of the analysis cannot be presented as 'easy' narrative." This problem is attributable, in part, to the fact that the book is actually composed of several major studies. Thus, each chapter from 2 through 5 is a presentation of the results of a different major study. This serves to make the narrative difficult to follow and somewhat labored.

Another weakness of the book is the failure to place the research within a theoretical framework. Although Franklin and Ripley do a good job of placing their research within a policy framework by relating it to a stream of previous research done on CETA and related policies, no effort is made to ground the analysis in a particular theoretical construct in American politics. For example, some mention could have been made of pluralist theory with which the authors' findings seem very compatible. Further, only in the last chapter is an effort made to link the research with general knowledge of the American political and policy process. Had a theoretical context or framework for the research been placed earlier in the book, it could have been followed with a discussion throughout the analysis chapters of how the specific findings were or were not supportive of the theoretical framework. The inclusion of such a theoretical framework would not only have improved the book substantially, but also would have helped the narrative flow better in the data analytic chapters by providing the appropriate linkage for a discussion of several studies in those chapters.

All in all, *CETA* is a significant book. It will undoubtedly make a worthwhile contribution to the policy studies literature and is likely to become a leading reference on CETA and one of the leading works on policy implementation.

HUEY L. PERRY

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Hidden Spending: The Politics of Federal Credit Programs. By Dennis S. Ippolito. (Chapel Hill: University of North Carolina Press, 1984. Pp. xiv + 169. $19.95, cloth; $9.95, paper.)

This is an indispensable volume for those seeking a more complete understanding of the fiscal dilemma that now confronts the United States. The long-standing incrementalism of the partisan-mutual adjustors and their disjointed interplay with their fellows in the low-visibility subgovernments that dominate the manifold spheres of public policy have taken ingenious forms. The cumulative results of these multiple, expanding, and now intersecting policy trends have produced what Richard Rose has aptly termed a state of inertia, one quite possibly beyond sensible management and having, to put the matter euphemistically, "extremely variable" consequences.

This dismal story is recorded daily in the press under some variation of the budget deficit headline, with the focus most often placed on the conventional taxing and spending categories described in the standard texts on budgeting and public finance. The problem of making sense of ordinary budget and fiscal policy is difficult enough—political propaganda, however jejune, obfuscates what are already inherently complicated issues—but political scientists and economists have written the scholarly studies that enable those of us in the classroom to clarify the central questions involved. Instructors in public policy, budgeting, and finance courses have also been aware of the related and obscure happenings regarding federal credit programs but have lacked a convenient and comprehensive treatment of the subject.

Fragmentary facts and insights into these programs have been available, but it has remained for Ippolito to organize them more systematically and to forge them into a coherent political economy framework. His book has the added virtue of being well written. As such, it can serve diverse constituencies: undergraduate and graduate students in political science and economics, their professors, journalists, policy activists, and politicians.

The composition, size, and trends of the federal credit programs are revealed in the book's 33 tables, seven figures, and several brief case studies. They deserve and receive scrutiny, even though, as Ippolito makes clear, few successful institutional efforts have been made to deal with their implications—economic effects are at best vaguely understood; no ranking by merit occurs, either among credit programs themselves or between credit and on-budget programs; credit is extended with few if any controls; accounting practices confuse rather than clarify the scope and content of credit activity. It is not that credit budgeting and accounting reform is so intractable or recondite a problem that intelligent people have been unable to fashion solutions. Indeed, both the Carter and Ford administrations sought, albeit somewhat half-heartedly, to do so. And the Reagan administration, with still very qualified success, has mounted a more resolute challenge. It is rather that the credit beneficiary groups (in agriculture, housing, the export sector, small business, higher education to name some major areas) recognize that their dispensations could not pass muster in an open budget process. Reform has thus been fought with tenacity and considerable success by those who accrue the rewards from hidden spending. One of Ippolito's controlling themes about credit programs makes their major objective and strategic behavior understandable. "They have been used to shelter programs that otherwise could not compete successfully for scarce resources" (p. xiii). Spending is concealed for good reason.

Ippolito is not talking small potatoes. By 1982, there existed some 350 direct loan or loan guarantee programs. (Loan should often be rendered "loan," because many are never intended to be fully collected or collected at all.) Some $500 billion in credit was outstanding by 1982, amounting, according to Alice Rivlin, to "one of every eight dollars expended by federal agencies." Ippolito emphasizes that "All credit programs include subsidies. . . . The effective result is a cash grant to the borrower" (p. 4). Lest one believe that the credit sector is stable or shrinking in relative terms, Ippolito shows that its growth rate has at least been equal to the rise in conventional federal spending in recent years and may have exceeded it. Ippolito offers several commendable reform proposals, although he is far too sophisticated with respect to the underlying incentives at work to expect easy acceptance of fundamental ones.

By bringing a hidden subject out of the closet in a challenging book that will surely become mandatory reading in many academic settings, Ippolito has made a major contribution to our understanding of an irresponsible and disturbing failure in collective choice. By enhancing public awareness of the dimensions of that failure, he will have placed renewed attention on a fundamental question for democrats: whether those who control our political institutions can be induced to act prudently with the nation's purse by comparing and ranking public purposes during daylight hours.

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The Winning Ticket: Daley, the Chicago Machine, and Illinois Politics. By Melvin A. Kahn and Frances J. Majors. (New York: Praeger Publishers, 1984. Pp. xx + 280. $29.95.)

The title of Kahn and Majors' book is an apt description of its content. The authors—using a case study approach under the methodological rubric of participant observer—analyze the nominating process in Illinois party politics with a focus on the influence of the Daley machine in Chicago and its linkage to party politics throughout the state. An attempt is made to focus on the nominating process in order to ascertain new insights about its structure and functioning. Concomitantly the authors attempt to generate questions about the future of Chicago politics in light of the changing character and fortunes of the political machine.

In this study the social science concepts of *gemeinschaft-gesellschaft* adapted by Ferdinard Tonnies and "stratarchy" articulated by Samuel Eldersveld are employed as heuristic tools to provide an analytical basis for examining the power relationships that previously existed within the Cook County Democratic party organization and between it and the state Democratic party. The authors suggest that the *gemeinschaft-gesellschaft* dichotomy and the concept of power distribution called "stratarchy" provide a useful framework for understanding the structural variables relevant to humanity's struggle to survive, politically and otherwise, in the modern world. On one hand, citizens must perform in the *gesellschaft* milieu, an urban and industrial society characterized by impersonality, preoccupation with economic struggle, change, and social relationships based upon law. In short, it is a cold, uncaring world dominated by depersonalized relationships and formal contracts. However the authors contend that modern men seek the intimacy, psychological comfort, and economic simplicity of *gemeinschaft.* They seek the small, folk community characterized by emphases on the personalized relationship of family, kin, and neighbors, subordination of economic competition to group cooperation, and social relationships based upon custom (p. xvii).

Kahn and Majors also maintain that the political machine, both in its everyday operations and in the important nomination function, carefully balances the worlds of *gemeinschaft* and *gesellschaft:* It pays attention to its members' desire for financial status and for professional prestige—acknowledging the special needs of organized labor, business and industry, and the institutions of formal government. And at the same time the Machine also pays careful attention to the *gemeinschaft*-oriented identifications of many Chicagoans who live in and identify with their respective ethnic communities. The point is made that it is necessary for the Machine to acknowledge the multiplicity of interests in Chicago as well as the *gemeinschaft*-rooted party supporters in both Cook County and downstate that make unavoidable its recognition of its decentralized power structure.

Although well-written but somewhat journalistic, the greatest weakness of the book is its failure to clearly show how the principal analytical concepts are directly related to theory. Perhaps this flaw is more evident with respect to the concepts of *gemeinschaft-gesellschaft* than it is with respect to "stratarchy." For example, it is not clear—even if one understands the concepts of *gemeinschaft-gesellschaft*—how these concepts are useful for providing law-like propositions amenable to building a more comprehensive explanatory system. On the other hand, the attempt by Kahn to receive the party nomination for candidate for state superintendent of public instruction, as well as the manner in which the Machine dealt with the competing claims and services imposed by the diverse ethnic coalitions and the resulting reciprocal support structures more clearly demonstrate the operation of the concept of "stratarchy."

Another troubling aspect of this work is Kahn and Majors' failure to mention or make use of more common concepts associated with party politics and elections. For example, a more explicit discussion of pluralism, group theory, role theory, or some variation on the power paradigms would have given the work more balance and thoroughness.

The authors' epilogues seem to provide a springboard for the analysis of political power in post-Daley Chicago. In this respect two developments are crucial: a growing black population with a stronger power base and a better organized, more active, reform-oriented white liberal bloc (p. 220). Despite my previous criticisms, however, Kahn and Majors have produced a well-written, readable book on practical politics.

OLIVER JONES, JR.

Florida A & M University

Congress and National Energy Policy. By James Everett Katz. (New Brunswick, N.J.: Transaction Books, 1984. Pp. vii + 287. $29.95.)

James Everett Katz's *Congress and National Energy Policy* is a useful addition to the literature on Congress, energy policy, and the national political process. Although it concentrates mostly on the policy process, some sections deal with aspects of policy analysis.

Part 1 reviews the developments in national energy policy in the 1970s and early 1980s. Most of the chapters focus on the proposals of the Nixon, Ford, and Carter administrations and Congress' reaction to their policy programs. Katz also mentions initiatives members of Congress developed in energy policy. This part of the book not only provides an historical sketch of energy policy in the 1970s, but also provides an analysis of why certain proposals passed while others failed in Congress. Katz, for instance, suggests reasons why several parts of Carter's National Energy Plan were either defeated or significantly altered. Part 1 concludes with a chapter on the Reagan administration's plans to lessen the federal government's role in energy policy.

Part 2 examines the implications of the recent energy policy process in relation to the national political process. Katz presents some insightful comments on what can be learned about how Congress functions in the political process by examining the developments in energy policy.

Katz presents a mixed review of Congress' ability to handle energy policy. He suggests that Congress became more active in energy policy and was better prepared to take a more independent course from the executive branch partly because of improved staff resources. Congress brought more groups and interests into the debate and resisted some presidential energy proposals that would have put the U.S. in a weakened energy position. On the other hand, Katz believes that Congress lacks an adequate organization system to develop sound national energy policy. "Although it has now established an influential presence in energy policy and can be expected to maintain it, Congress is poorly equipped for this responsibility, despite advances made in the integration of its operations. Thus far, few congressmen have tackled the organizational and procedural changes required to fully participate in the setting and evaluating of the nation's energy policy" (pp. 201-202).

One of the strongest parts of the book is a section on organizational theory. Katz presents an excellent framework for analyzing congressional behavior by relating Congress' actions in energy policy to some of the literature on organizational theory.

A weakness of the book is the confusion at times over what is the central focus of the analysis. In some sections the central focus seems to be on the national political process related to energy policy rather than Congress and national energy policy. The chapter on the Reagan administration is a good example of this problem. Although the chapter discusses some congressional activities on energy policy after 1980, most of the discussion relates to the energy proposals of the administration.

LARRY M. SCHWAB

John Carroll University

U.S. National Health Policy: An Analysis of the Federal Role. By Jennie Jacobs Kronenfeld and Marcia Lynn Whicker. (New York: Praeger Publishers, 1984. Pp. xii + 274. $29.95.)

Nowhere are the limits of the American welfare state so transparent as in health policy. Despite growing financial commitments, it is hard to say with any certainty that we are a healthier people today than we were two decades ago. Rather, America faces a looming health care crisis born of changing disease patterns associated with an aging population, an increasingly capital-intensive approach to medicine, and rising public expectations about equal access to quality health services. Federal action and inaction have played a large part in stoking these fires, and Kronenfeld and Whicker provide a serviceable summary of federal health policies and a discussion of some of the issues raised by them. But the authors' ambitions are too modest, and they do not always deliver on what they do promise.

The authors are at their best when describing the federal statutes and organizations that deal with health care. Overall, they find that federal agencies and programs in health research, planning, and services are disorganized, uncoordinated, and often incoherent. The account of the convoluted twists and turns taken in the development of the Public Health Service is particularly interesting. There, Kronenfeld and Whicker convincingly show that the constant reorganization of the federal bureaucracy has seriously undermined the development of coherent policy.

But this book has several serious limitations. Most important, the authors focus on the wrong issues. From a comparative perspective, the American health care system is distinctive because of its political and institutional arrangements, specifically the power and autonomy of private actors, the absence of central planning, and the use of public monies as supplements and subsidies to private programs. Kronenfeld and Whicker, however, focus on an ad hoc collection of technical and economic issues, specifically American "technological supremacy" (p. 1), cost inflation, and the evaluation "conundrum" (p. 11). Even when they themselves argue that it does not make sense to analyze health care in terms of markets, they organize their discussion of resource allocation into chapters on, respectively, the demand for, and the supply of, health care. Thus, as in most microeconomic treatments of social policy, politics and economics are not brought together and considered as a whole.

As a result, Kronenfeld and Whicker cannot explain what they describe, particularly the underdevelopment of government action in this area. They suggest that the health policy process reflects the general characteristics of social policymaking in the U.S.: a liberal capitalist ideology, fragmented government institutions, and the prevalence of iron triangles. But they do not demonstrate how policy outcomes reflect these realities *in this case.* How do the medical profession, the hospital and pharmaceutical industries, the various federal agencies and Congressional committees specialized to this problem, and the peak associations of labor and business shape health policy? The authors offer no answers to this question. Remarkably, despite the obvious role of doctors and hospitals in the development of health policy, there are only four references to the American Medical Association in the entire work; none to the American Hospital Association. Conflicts among health-related agencies are left unanalyzed. Why have both the Congress and the executive been unable or unwilling to control health care costs? Why has progress toward getting medical care to poor people and to people in rural areas been limited?

Finally, as simple description, the policy summary leaves out too much. It overemphasizes statutes, total spending levels, and formal agency organization, and underemphasizes both health policy implementation and the finer details of government action. Total federal expenditures are not displayed and broken down for analysis. Agency programs are not described in any significant detail. To its credit, the book brings together enough materials to raise important questions. But, in the end, we are left to wonder why the federal government has done such a poor job in this area to date, where we might look for new kinds of government programs and the likely trajectory of policy beyond the near term.

CHARLES NOBLE

Rutgers University

Public Opinion and Foreign Policy: America's China Policy, 1949-1979. By Leonard A. Kusnitz. (Westport, Conn.: Greenwood Press, 1984. Pp. xii + 191. $29.95.)

Kusnitz's study attempts to determine the relationship between American public opinion and the formation of U.S. foreign policy. As his case study, he uses the development of America's China policy between 1949 and 1979 and finds that: (1) media coverage, presidential style, and international crises affect the public's opinion of American foreign policy; (2) in formulating their foreign policies, presidents had to prepare the public for foreign policy changes; and (3) public opinion expresses the extent to which a president's foreign policy is vague or discrete.

Media coverage was an important factor during the McCarthy era, when the American press held the Democratic leadership responsible, for "losing China to the Communists." Two decades later when President Nixon visited Peking, the American media presented the People's Republic of China favorably.

Presidential style is an important factor in directing the public's attention to a particular issue and enabling the president to gain support for his foreign policy. For example, Kusnitz notes that in 1950 President Truman heightened public awareness by reiterating a noninterventionist approach to the Chinese civil war. As a result, a five-to-three majority of the American public supported Truman's "hands off" policy toward Formosa. Kusnitz focuses on the Korean War as the major international crisis affecting the public's opinion of America's China policy. At various times, the Korean War limited and heightened the American public's interest in defending Taiwan.

Both Eisenhower and Nixon prepared the public for changes in America's foreign policy toward China. This enabled Eisenhower to effectively disregard public opinion and not offer substantial military aid to the Taiwan Nationalists. Nixon insisted that the Shanghai Communique include cultural and educational exchanges so that he could prove to the American people that China was interested in establishing friendly relations. In contrast, President Carter had to change his China policy because he had not prepared Congress or the American people for an unofficial relationship with Taiwan.

Public opinion polls reflect the difficulty that American administrations have had with defining a stable and reasonably coherent China policy. Kusnitz presents numerous examples where public opinion is vague about the extent to which the United States should develop its relationship with China. For example, in 1954, a majority of Americans did not want to improve relations with China. Less than one year later, public opinion had dramatically changed with an overwhelming majority favoring improved relations.

Kusnitz acknowledges that measuring public opinion over time depends on the wording of questions as well as events. He states that public opinion polling may increase one's awareness of an issue and that drastic changes in public opinion indicate changes in the public's attentiveness to a particular issue.

In comparing the poll results to foreign policy, Kusnitz finds situations where opinion changed both before and after policy. This finding is valuable because previous studies indicated that public opinion was not a significant factor in the formulation of foreign policy.

Although Kusnitz presents some useful information about the Eisenhower administration, the title of his book is a misnomer because most of his information deals with the Eisenhower administration, not 1949 through 1979. Kusnitz effectively argues that the Eisenhower administration warrants more attention because America's China policy was formulated at that time. However, I think that he stretches his argument too far when he implies that public opinion in 1979 was affected by the "Democrats loss of China" in 1949 (pp. 176-177).

PEGGY S. CHRISTOFF

Boston University

State Policies and the Aging: Sources, Trends, and Options. By William W. Lammers and David Klingman. (Lexington, Mass.: D. C. Heath, 1984. Pp. xii + 253. $32.00.)

William W. Lammers and David Klingman have written an extensive study of changing state policy commitments for health and long-term care, income maintenance, social services, and consumer protection for the aging. Their research effort consists of two parts, one an aggregate-level study of the 50 states and the other, interview studies in eight selected states.

The aggregate-level study consists of the following major dependent variables: health and long-term care, income maintenance, social services, and consumer protection. Each one of these variables is represented by a variety of indicators and composite factors, for example, Medicaid expenditures and programs and nursing home regulations are used for health and long-term care. The major independent variables are aging advocacy, general policy liberalism, fiscal capacity, political capacity, political openness, socioeconomic development, and status of the aging population. A variety of indicators and composite factors represent the independent variables. For socioeconomic development there is a need factor, a socioeconomic status factor, an industrialization-urbanization factor, Sullivan's index of cultural diversity, and an index based upon Elazar's three types of political cultures. All of these variables when available are evaluated at five time points: 1955, 1960, 1965, 1970, and 1975. The authors used the computer program package Statistical Analysis System (SAS) to compute composite factors, simple correlation analysis, and step-wise multiple regression.

An example of their effort may be found in their correlation analysis of health and long-term care. They argue that the controversial nature of Medicaid makes it a better indicator of commitment than Medicare. With this in mind they found that the most significant variables related to commitment in the introductory years of Medicaid to be the socioeconomic ones, industrialization-urbanization, and cultural diversity. The magnitude of needy groups had a significant but negative correlation. Party competition, voter turnout, and general policy liberalism were strongly correlated with the dependent variables. Tax effort and the powers of the governor were also significant. By 1970 they found that industrialization-urbanization and need continued to be strong. General policy liberalism and tax effort showed strong correlations with the Medicaid effort. For political capacity, centralization was correlated significantly only with optional services and that inversely rather than directly. According to the authors, that was because of the fact that many southern states were highly centralized but offered few optional Medicaid services. They suggest that overall public-sector development may be more important for political capacity than state centralization. For 1975, they found that cultural diversity continued to be strong, while industrialization-urbanization was not as strong as in the preceding years. The need factor was also weaker in 1975. Increases in the aging population and net migration of the aging were significant but interestingly showed that the aging were not moving to states with generous Medicaid program benefits. Political variables such as party competition, voter turnout, and the role of the governor were not particularly strong. This type of analysis continues for the other variables along with other statistical tests.

The second half of Lammers and Klingman's study consists of interviews with politicians, legislators, and bureaucrats in eight states. The eight states selected to provide diversity in policy effort, percentage of aging population, and general policy liberalism were California, Florida, Iowa, Maine, Minnesota, North Carolina, Ohio, and Washington. The eight case studies provide useful sketches of the states' political cultures and aging policies, however the authors are much better at describing policy trends as measured by their aggregate data analysis than in explaining the whys behind policy commitments and variations among the states.

Overall this is a significant study for specialists in aging policy and state government. Those with a command of basic statistical methods will enjoy this book, others should approach with caution. Lammers and Klingman should be congratulated for pulling this data into one useful source.

JOHN W. CRITZER

Mount Saint Mary's College

Modern Presidents and the Presidency. Edited by Marc Landy. (Lexington, Mass.: D. C. Heath, 1985. Pp. xx + 227. $25.00, cloth; $13.00, paper.)

This book consists of a set of revised and edited papers and related comments, the original versions of which were presented at a symposium on American politics held in late 1983 at Boston College. The participants in this program included a number of prominent academics, journalists, attorneys, and professional politicians. Most of the papers provide a partial, if not rather skewed, analysis of the major presidents of the post-1933 period (those not treated even briefly are Kennedy, Ford, and Carter); two deal with the concept of the "rhetorical" president; and two focus on the increasingly important institutionalized presidency. The majority of the papers were prepared by academics, a few by journalists or free-lance writers, and one, that on the Nixon presidency, by former Arizona Congressman and Republican minority leader John J. Rhodes (to the impartial observer, a politically questionable selection).

As many a scholar has ruefully noted, most edited works are of an uneven nature, some more than others. But this book, edited by Marc Landy, is unfortunately of a generally poor quality. Very few of the papers and comments appear to reflect much thought or research (although one exception is Thomas Cronin's contribution to the section on the institutionalized presidency). Many are of a highly personal nature or are filled with interesting, amusing, or sometimes trivial anecdotes. For instance, one writer recounts how in 1944 during the course of his research on Eisenhower he "spent a night in Telegraph Cottage [a secret World War II hideaway] sleeping in Ike's bed and Ike's pajamas." A number of the other contributors benignly engage in something bordering on hero worship. One scholar, in fact, devotes part of his chapter to describing Lyndon B. Johnson's "soul" and "severe moral creed." And one would have to look hard elsewhere in this volume to find much which is really new or important.

In short, there is relatively little in this work that has not already been better stated in many of the other more substantial analyses of the American presidency.

PHILIP H. BURCH

Rutgers University

Religion and the Presidential Election. By Paul Lopatto. (New York: Praeger Publishers, 1985. Pp. xii + 179. $32.95.)

The Evangelical Voter: Religion and Politics in America. By Stuart Rothenberg and Frank Newport. (Washington: Institute for Government and Politics of the Free Congress Research and Education Foundation, 1984. Pp. vi + 176. $5.95, paper.)

These books reflect the dramatic increase in research interest in religion and politics we have witnessed in recent years. In the past many social scientists were reluctant to attribute importance to religious forces because it was generally held that American ideologies have a purely secular base and are not grounded in theological dogma. Recent events including the rise of such groups as the Moral Majority and Catholic Church activity on the abortion issue have provided the impetus for a reevaluation of the role of religion in the political arena.

The two books offer distinct approaches to defining religious groups and measuring religiosity. Lopatto classifies members of religious denominations in terms of orthodoxy (belief in God, divinity of Jesus, biblical miracles, and life after death) and ethicalism (the belief that "doing good for others" and "loving thy neighbor" is a requirement for salvation). Based on the two dimensions, respondents included in the SRC's 1956-1980 election year surveys are classified as conservative, moderate, or liberal Protestants; Catholics are treated separately.

The authors of the *Evangelical Voter* take a different approach. They include in their national sample respondents who gave affirmative answers to questions concerning their belief in the divinity of Jesus and who have had a "born again" experience. Many Catholics are included in their sample of evangelicals. The independent variables used throughout the book are six separate measures of religiosity ranging from church attendance to awareness of a fundamentalist publication.

Using quite different samples and operationalizations of religious beliefs, both books examine the influence of religion on partisan identification, voting behavior, and issue positions. Lopatto provides the most useful information on electoral behavior. His data showed that the influence of religion on voting behavior varies considerably from one election to the next. The most volatile groups are the conservative Protestants and Catholics. Because of their relatively low socioeconomic status, the normal vote of conservative Protestants is Democratic. However, when religion and social issues are highly salient, as they were in 1972, conservative Protestants are especially likely to respond to short-term forces. There appears to be a dramatic decline in Catholic support for Democratic candidates. The decline really began in 1968 and is especially noteworthy among younger voters. The substantial economic progress of Catholics threatens their historic link with the Democratic party.

Two conclusions regarding electoral behavior, based on the findings of both books, are especially interesting. The first is that religion played a much smaller role in the 1980 election than many observers believed. Rothenberg and Newport clearly show that Reagan did not receive overwhelming support from evangelical fundamentalists. The second interesting finding is that candidates themselves determine the extent to which religion plays a critical role in elections.

The *Evangelical Voter* presents the most interesting data on issue positions. Rothenberg and Newport analyze a wide variety of issue items not included in the SRC election surveys. Some of the findings regarding evangelicals are surprising. For example, evangelicals split fairly evenly on the questions of a nuclear freeze and tuition tax credits. Yet as expected, religiosity is for the most part positively associated with support for military aid to Central American allies, increased defense spending, and prayer in schools and is negatively associated with support for abortion rights, the ERA, and the dissemination of birth control information. Although it is not surprising that religious values correlate with many political opinions, it is surprising that there is a great deal of opinion diversity in religious groups that might be expected to be more homogenous; the sample of evangelicals exhibits a great deal of disagreement on such issues as tuition tax credits and even abortion. The influence of religion is mitigated in many instances by such variables as race, region, and education. In sum, the fundamentalist groups are not nearly as monolithic in political viewpoint as many religious leaders and journalists would lead us to expect.

Although the two books share some concerns, in several respects they each offer unique contributions to the literature. Lopatto focuses on the influence of religion in the political arena. He concludes that conservative Protestants are experiencing threats to their lifestyle, but their personal economic concerns prevent religious right elites from mobilizing the group. Rothenberg and Newport questioned their respondents about the influence of clergy and religious broadcasting on their political views. They find that the local church is not being used as a forum for political expression, but that televised religious programs have the potential for political influence of the very religious.

The authors of both books conclude that religious forces are important in the contemporary political arena. However, a more appropriate conclusion might be that religion *can* be influential. Rothenberg and Newport find that evangelicals are nearly as varied as the public at large, and Lopatto finds that candidates play a key role in determining the role of religion in elections. Both books have their limitations. By excluding all "nonevangelicals" from analysis, Rothenberg and Newport lose the opportunity to make some meaningful comparisons. Although Lopatto has the advantage of longitudinal data, his almost exclusive use of SRC election surveys limits his ability to operationalize religious variables and the variety of political issues he can analyze. Readers will find the *Evangelical Voter* to be more timely, but both books deserve to be read because they make valuable contributions to the literature on religion and political behavior.

JAMES M. CARLSON

Providence College

Regulation: Politics, Bureaucracy, and Economics. By Kenneth V. Meier. (New York: St. Martin's Press, 1985. Pp. xviii + 334. $32.50.)

This volume is an excellent analysis of government regulation of the private sector in the American political system. Focusing primarily on regulatory efforts at the national level, Kenneth V. Meier explores in depth the nature of the regulatory process. The areas of regulation examined are financial institutions, consumer protection, agriculture, environment, occupations, workplace safety and health, and antitrust policy.

In addition to the mechanics of regulation in each of the policy areas, Meier presents the history of government involvement in the issue area, key actors, conflicting values, and an assessment of who benefits from the type of government regulation in force. Contrary to much of the popular discussion of regulation that seldom finds value in various regulatory structures, Meier does not hesitate to indicate what he sees as the positive social role that regulation plays in such areas as the environment and worker health and safety. However, although the book contains normative statements about aspects of the regulatory process, it is far more than a work focusing on the positive and negative aspects of regulation. Instead, it is primarily an effort to focus on why we have regulation by examining historic and other factors that bear on the establishment and development of regulation of a particular sector of American society.

Even though this book contains a great deal of information about specific regulatory areas, it will be of value to the scholar of regulation because it presents an analytical framework and numerous propositions concerning the nature of regulation. In addition, Meier marshals significant data to challenge some of the long-standing notions of regulation. For example, he presents reasonably conclusive evidence that the concept of "capture" of regulatory agencies by the regulated groups is not an inevitable outcome for all areas of regulation as has been suggested by Marver Bernstein and others.

One of the strengths of the book is that it is able to focus on the role of bureaucracy in regulation. In this regard, Meier convincingly states that regulatory bureaucracies tend to develop organizational ideologies that provide an orientation to their work able to resist and adapt to outside pressures. For example, the discussion of relationship between the Reagan Environmental Protection Agency appointees and the civil service professionals is instructive. Thus over a relatively short period of time, the EPA bureaucracy, with the assistance of congressional allies and the media, was able to play a significant role in the ousting of Anne Burford, Rita Lavelle, and others whose far more lax approach to the enforcement of environmental laws and regulations conflicted greatly with the existing organizational ethos.

In summary, Meier has produced an excellent book on an important and growing aspect of government in our society. The discussion is up to date both in terms of the literature utilized and the events described. Further, although the analytic concepts of political science dominate the explanation of regulatory phenomena, Meier is sufficiently versed in the explanatory contributions of economics and law so that these perspectives are added as well. The thoughtful framework provided by the author makes *Regulation* appropriate reading for the specialist, while it is also appropriate for advanced undergraduate and graduate courses dealing with regulation.

THOMAS VOCINO

Auburn University at Montgomery

Dangerous Offenders: The Elusive Target of Justice. By Mark H. Moore et al. (Cambridge, Mass.: Harvard University Press, 1984. Pp. vi + 252. $20.00.)

Evidence that a small percentage of persons account for over half the serious violent crime in America has led many agencies to concentrate attention on these dangerous offenders. This book probes those efforts. The authors present a coherent, well-documented treatise first seeking a framework for evaluating dangerous offender policies and second discussing specific policies at particular stages of the criminal justice system. The book can be read methodologically as an excellent example of comprehensive policy analysis and/or substantively for its thorough examination of the idea the criminal justice system should focus on dangerous offenders.

The first chapter looks at various dimensions of the "crime problem," and concludes the dangerous offender is never out of the picture in any of them (p. 22). The second chapter gives the concept of focusing on dangerous offenders a theoretical and empirical basis. The research core of the book, this chapter yields two appendixes—one on estimating the rate of offending of the average street criminal, and one spelling out an indirect measure of system bias (intended to bolster the authors' argument that the present operation of the criminal justice system lessens rather than exaggerates differences among offenders). The chapter offers a practical operational definition of a dangerous offender (p. 60) based on a record of offending showing "violence, a high rate, and persistance" (p. 183).

A third chapter considers an array of objections based on concepts of justice, practical values, and concern about broader ideological and institutional effects that generally challenge the wisdom of so-called "selective policies." Admitting some of the objections are at least debatable, the authors argue that they are best considered in the context of specific proposals, then serving to mark areas of concern or uncertainty rather than standing as barriers to selective policies. The next four chapters consider such policy proposals as selective sentencing, pretrial detention, prosecuting, and investigation. The sequence of these four chapters is determined by the authors' recognition that the first represents an area with traditional policies of selectivity and the last an area still in a state of experimentation. All but pretrial detention are recommended as areas in which new selective policies offer significant opportunities for practical gains and improved justice. And while providing a persuasive argument in support of extended prison terms for dangerous offenders (p. 194), the authors conclude the "potential for practical gains and enhanced justice . . . is likely to be greater at the front end of the criminal justice system" (p. 185).

Finally, before their summary chapter the authors insert a chapter that concludes with a recommendation that the federal government develop a fully computerized system linking local, state, and national records (p. 180), recognizing that much of what is discussed and recommended in the book depends on the quality and availability of records.

The scholarly style of this book directs the reader to possible points of contention. For example, the authors discuss the fact that privacy concerns and traditional views of juvenile records would have to give way in order to realize system-wide, offender-based record-keeping proposals. Likewise, they choose an operational basis for the dangerous offender that is based on past performance rather than a prediction of future criminality and attempt to base the ultimate disposition policy of selective incapacitation for the dangerous offender on blameworthiness factors rather than utilitarian concepts of deterrence or rehabilitation. They admit that the effective selective policies for which they argue still ought not to be considered the cure for all or even most of the problems of the criminal justice system. And as the authors proclaim in a section reviewing dangerous offender definitions from different state jurisdictions: "We can offer conceptual guidance, establish some limits on suitable definitions, and assess current definitions, but we cannot decisively resolve the issues through sheer force of logic and evidence" (p. 54).

RICHARD G. FREY

State University of New York College at Brockport

Immigration: The Beleaguered Bureaucracy. By Milton D. Morris. (Washington, D.C.: Brookings Institution, 1985. Pp. x + 150. $22.95, cloth; $8.95, paper.)

Milton D. Morris has produced an ultimately unsatisfying book about immigration policy in general and the Immigation and Naturalization Service (INS) in particular. This book serves as an administrative case study of what nearly everyone (Morris included) deems one of the United States' least effective agencies. It also doubles as a prescriptive analysis of immigration policy from the agency's perspective. These tasks finally overburden the slender volume's narrow descriptive approach. Moreover, there is something odd about delivering a policy brief for an agency one "praises with faint damns."

To repeat, this is a case study. Morris neither develops nor explicitly applies any systematic generalizations about administrative behavior or policy development. This is not necessarily a fault. Description sets up prescription here, and Morris hopes to contribute to a single policy debate that principally involves a single agency.

Even at this level, Morris's description proves unduly narrow. He informs us, quite correctly, that one of the most serious problems that the INS faces is public uncertainty about immigration policy and the methods and consequences of enforcing it. But he lists the various conflicting or ambiguous demands without any serious effort to evaluate them. He attempts no review of the mounting literature on the economic impact of undocumented workers and their dependents. Nor does he discuss noneconomic claims such as the increasingly prevalent notion that illegal immigration per se undermines a nation's sovereignty. These omissions would not condemn a book that focused exclusively on administrative effectiveness. Divergent claims and self-contradictory expectations might fade into the unfocused background of the INS's environment.

But Morris prescribes policy at the end of this book: more INS funding, more computers at INS, a much larger Border Patrol, and an INS public relations campaign to persuade us to like the "beleaguered bureaucracy" a bit better. These recommendations presuppose, without argument, that the INS and its allies are correct in their policy assessments. They simply cannot be inferred from the brief history of immigration policy and the bland catalog of the INS's tribulations that proceed them.

Thus the prescription fails to convince because the book comes alive neither as a policy analysis nor as an administrative case study. Only when Morris describes INS record-keeping in a computer age—yellowed papers gushing forth from scattered cardboard boxes—does he begin to capture the anachronistic slovenliness of the "beleaguered bureaucracy." Beyond the safe confines of a plea for more computers, Morris takes a view of the INS that seems sanitized beyond the demands of academic decorum. He ignores the agency's relatively recent flirtations with nativist groups and dismisses illegal acts of collusion with labor-hungry employers as practices that ended with the Second World War. Finally, he never mentions the widespread and unmasked hostility President Carter provoked within the INS by his appointment of a Mexican American Commissioner.

The argument here is not that Morris has failed to titillate, just as there is nothing foul about a scholar concluding that immigration restrictionists are right. The problem is that this barebones and ultimately slanted description of immigration policy and enforcement simply does not provide us with the information and insights we need in order to evaluate Morris's prescriptions. Thoughtful persons must at least consider the argument that immigration is less of a problem than many of us believe as well as the possibility that the INS cannot be reformed by love, money or computers.

BERT C. BUZAN

California State University, Fullerton

Losing Ground: American Social Policy, 1950-1980. By Charles Murray. (New York: Basic Books, 1984. Pp. ix + *323. $23.95.)*

Losing Ground is a lucidly written, extensively documented, tightly argued assault on the assumptions and policies of the Great Society. It is intellectually provocative and emotionally powerful. It is also seriously flawed—distorted as description, incomplete as explanation, and lethal as prescription.

The heart of *Losing Ground* is an impressive marshalling of data that Murray believes demonstrates that, beginning in the mid-1960s, the life of the poor, particularly black poor, took an abrupt and dramatic turn for the worse. According to Murray, in education, unemployment, crime, family structure, and poverty, conditions got "much worse than they 'should have gotten' under the economic and social conditions that prevailed in the society at large" (p. 135).

Murray's explanation? The social progress of the 1950s and early 1960s was reversed by the Great Society itself. The very programs intended to ameliorate the conditions of the poor exacerbated them. There was a "paradigm shift" (p. 41) in intellectual assumptions about poverty; the conventional wisdom became "the system is to blame" (p. 45). New "rules of the game" interacted with changes in status rewards regarding work and family to produce radical alterations in the attitudes and behavior of the poor. Thus, "increasing unemployment among the young, increased dropout from the labor force, higher rates of illegitimacy and welfare dependency"—these and much more were "rational [if pathological] responses to changes in the rules of the game" (pp. 154-155).

What doomed the Great Society, according to Murray, is what might be termed the "Law of Perverse Consequences": any program designed to alleviate a social ill invariably creates or maintains the conditions for the ill to proliferate. If being poor is made a qualification for a government benefit, if, in other words, poverty is to be officially rewarded, many will choose to become or remain poor.

This view has the advantage of not patronizing the poor as too stupid to respond rationally to major reward structures and has found favor with many prominent officials and thinkers over the centuries. Yet it has always been open to the charge of being simplistic. Are rational calculations really so deterministic of human action, it is asked, and what of the community's responsibility toward its less fortunate members? Too many liberals have entered this briar patch of questions, Murray believes, and have never returned.

Instead, he proposes a truly radical social agenda: the elimination of every federal benefit program for the non-elderly except unemployment insurance. The Great Society and the New Deal would be scrapped; indeed, Murray would in some respects go back before the sixteenth-century Elizabethan Poor Laws. Under this "triage by self selection" (p. 231), the poor will seek, find, and hold jobs and form and rely on family because the "alternative is grim enough" (p. 228). Those "pauperized" by the withdrawal of government and unable to adapt to the new environment, he trusts, would be provided services by private charities and local taxpayers that would be "more generous, more humane, more wisely distributed and more effective" than those formerly subsidized by the federal government (pp. 229-230).

Although provocative, *Losing Ground* is not compelling. For one thing, Murray simply ignores substantial evidence of federal program success in the areas of employment, education, health, and poverty reduction. Also, some of the trends that Murray contends were caused by the War on Poverty, such as declining black male labor force participation, black family breakup and illegitimacy, and burgeoning welfare caseloads, began in the "Happy Days" era of the 1950s.

On several issues, moreover, Murray's categorical judgments are not persuasive. The evidence of welfare's effects on marriage and the family, for instance, is not at all clear cut. Nor do the Negative Income Tax experiments provide the "unambiguous" (p. 153) causal evidence of the negative effects of the welfare system on work and marriage that he claims.

More generally, Murray can be faulted for riding a good horse too far. For so heavy is his explanatory reliance upon incentive structures that he omits or summarily dismisses the additive and interactive impacts of macroeconomic, demographic, cultural, social, and psychological influences. To Murray, it is "not necessary to invoke" these other factors because he believes the phenomena he examines can be accounted for by the rational-choice approach (pp. 154-155). Simply, he violates a fundamental canon of science—correlation does not equal causality. As for his policy prescriptions, they are morally dubious. He makes no distinction between the "deserving" and "undeserving" poor; all will be officially neglected. Care of the poor has been part of the social contract; Murray would void that obligation.

Despite its deficiencies, *Losing Ground* is an impressive and important book. The Great Society, as Murray remainds us, was flawed; programs were often inefficient and wasteful, and sometimes inequitable; unanticipated negative consequences did occur. Many of the problems Murray raises—lax law enforcement, lowered educational standards, denial of individual responsibility, work and family disincentives in the welfare system—must be addressed.

The book has already engendered a vigorous debate over social policy; it will be an indispensable point of reference for polemicists and more dispassionate analysts. Indeed, *Losing Ground* may be a new *The Other America.* Just as Harrington's book forged a new conventional wisdom on who the poor are, why they are poor, and what society's responsibility to them are and provided the moral and empirical framework for the Great Society, *Losing Ground* has become the "social Bible" of the Reagan administration, providing an intellectual justification and a "moral legitimacy" for cuts in the social welfare budget, the ultimate goal being the dismantling of the welfare state. Properly dubious about the political feasibility of this goal, Murray nonetheless insists that "primitive questions about social policy ought periodically to be examined" (p. 196). Even those dissenting from his views would find it hard to quarrel with that.

DOUGLAS MUZZIO

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The Elections of 1984. Edited by Michael Nelson. (Washington: Congressional Quarterly Press, 1985. Pp. xiv + 318. $15.96, cloth; $10.95, paper.)

The genre of short-order election histories written by teams of political scientists is a relatively new one, having been around since the election of 1976. Still, the standards of evaluation for such books are reasonably well established. Although simply putting the convention wisdom of the journalists and pundits in a convenient place is, arguably, worth doing, one expects political scientists to do better.

Generally, these do. Thomas Patterson and Richard Davis discuss the media's excessive concern with the horserace aspects of campaigns, as well as the inordinate power of the media in a system where political parties are weak. Gary Jacobson deals with the behavior of strategic political elites in a system where presidential coattails are exceedingly short. Robert Birkby tells us that Reagan judicial appointments will reflect a variety of considerations and appeal to a variety of constituencies, even while reflecting the president's political philosophy. Gary Orren discusses how the rules of the nomination game influence the outcome, and Paul Quirk how economic policy is influenced both by elite views (particularly those of economists) and electoral constraints.

None of these themes is highly original. But there is no gainsaying their relevance in 1984, nor, if one has students to whom the Carter administration is a dim memory and President Ford ancient history, can one doubt the utility of these essays for instruction.

When these essays attempt to move beyond well-trodden disciplinal terrain and place the election of 1984 in a broader historical and theoretical context, the results are more mixed. Erwin Hargrove and Michael Nelson propound a theory of a cycle of politics and policy in which Reagan's first term (like with Johnson's, Roosevelt's, and Wilson's) constituted a "presidency of achievement" likely to be followed by a domestic policy stalemate in the second term. One is inherently suspicious of theories of cycles, but Hargrove and Nelson have produced one which does much less than the usual violence to the facts of history.

Benjamin Ginsberg and Martin Shefter analyze the 1984 election in terms of a conflict between the New Politics movement and a "reconstituted right." Although sensible in many details, the essay is marred by a rather vulgar, Beardian brand of economic determinism. Although bearing a superficial resemblance to Marxism, this sort of thinking actually vastly undervalues classes and ideologies. Theodore Lowi is as provocative as usual. He tends to downplay the scope of Reagan's policy achievements, a nice contrast to Nicholas Lemann, who believes that Reagan made some very fundamental changes. Unfortunately, Lowi takes seriously the possibility that Reagan may be able to establish an enduring majority coalition through the passage of tuition tax credits and adroit use of the military pork barrel.

One can raise a variety of (somewhat marginal) quibbles here. There is a fair amount of repetition in the essays. Several of the authors are too willing to accept at face value poll respondents' accounts of why they voted the way they did. Paul Light and Celinda Lake assert that Mondale cut Reagan's lead in half by his performance in the first debate—a conclusion inexplicable to anyone paying much attention to the polls. One could dispute Quirk: not only is he worried about the deficit (reasonably enough), but he also believes the voters behaved badly in rewarding Reagan for a growing economy. Quirk considers the fact of a growing economy "nearly irrelevant."

All in all, however, this book is a nice display of political science professionalism. We really do understand American politics better than the journalists, and it shows here.

JOHN MCADAMS

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Demand Processing and Performance in Public Service Agencies. By Stephen L. Percy and Eric J. Scott. (University: University of Alabama Press, 1985. Pp. xiv + 153. $20.00.)

This volume has its genesis in the workshop in political theory and policy analysis at Indiana University and was funded by a grant from the National Institute of Justice. The major premise is that "the initial interaction between service seekers and organizational gatekeepers has important implications for service delivery and therefore deserves close scrutiny" (p. 4).

As the title indicates, the major focus is on demand processing, which the authors define as "the system by which agencies receive and interpret service request signals and activate response" (p. 4), and how that phenomenon affects the performance of service deliverers. Toward that end, the authors develop a descriptive "process model" of demand processing that weaves together an assortment of concepts and possible behaviors.

The first three chapters develop the three core elements of demand processing. The first is gatekeeping, "the boundary-spanning activities through which agencies receive and interpret demand signals and initiate (or refuse) organizational response" (p. 16). Second is response coordination, through which "the initial response alternative selected by the gatekeeper is translated into concrete, service-rendering action" (p. 26). The third element is information, which essentially is the medium of exchange between the first two. The authors emphasize the importance of discretion to the exercise of these functions.

The middle chapters (4-8) apply the model to the Ft. Worth Police Department, with the data base being a series of interviews with police and citizens and information on more than 150,000 phone calls placed to the FWPD during 1980 and 1981.

The final chapter examines the impact that demand processing has on organizational performance and also makes several suggestions that summarily argue to elevate the prominence of demand processing as an ingredient in organizational performance. Critical impacts include the determination of who will and will not receive services, what kind of services will be provided for what types of requests, the gathering and coding of information, and direct interaction with citizens. By way of recommendations, Percy and Scott argue for greater attention to the pivotal role of demand processors, greater selectivity in hiring, more formal training, closer monitoring and supervision, and greater attention to information flow and the exercise of discretion.

The analysis is straightforward and successful in meeting its professed goals. It has obvious appeal for courses in criminal justice, yet is sufficiently versatile to be relevant to courses on any number of service delivery issues. As stated above the orientation is descriptive rather than analytical—a starting point rather than the final word. Interested readers doubtless should eagerly anticipate further work on this subject.

GERARD S. GRYSKI

Auburn University

Women, Public Opinion, and Politics: The Changing Political Attitudes of American Women. By Keith T. Poole and Harmon Zeigler. (New York: Longman, 1985. Pp. xii + 196. $25.00, cloth; $12.95, paper.)

With publication of this book, the study of gender differences in political behavior has clearly moved into a second phase. The first generation of scholars perforce had to justify their choice of topic by emphasizing those issues and attitudes on which men and women differ. Freed of the myth that gender is not a politically relevant characteristic, Poole and Zeigler can now question the scope of these differences.

Using ISPR national election data from 1968 to 1982, the authors focus on the mass attitudes of American women: the linkages between candidate preferences and attitudes on equality, the women's movement, and more general policy issues. They readily concede that women are not a conventional interest group. That is, women do not exhibit the group consciousness needed for bloc voting or mobilization by one umbrella organization. Instead, women are studied here as men have always been studied: as subgroups divided by social class, education, and occupation. From this perspective, the attitudinal gap between employed women and housewives was found to equal or exceed the better-known gender gap from 1980 to 1982.

The study is not without flaws. There are fascinating but intrusive digressions to more general questions of political science (e.g., the causes and meaning of Reagan's election in 1980). Their interpretations of findings at times appear to be questionable (e.g., an inference that feminism will be the crucial intervening link between increasing numbers of educated working women and changes in the division of labor in the home) or downright silly (e.g., the statement that unhappy single employed women are the ideal recruits for feminist groups). Finally, the authors alternate between a positive and sympathetic treatment of the women's movement and one that is uncomprehending and even hostile. For example, they argue that the feminist movement became more moderate as it matured and gained popular support. Yet elsewhere the movement is portrayed as radical, antifamily, and anticapitalist. Even so, because of the great care and clarity with which the authors present a very complex set of statistical findings, a wide range of readers can profit from this study.

Those active in the women's movement can take heart from the prognosis here. Although the sharp increases in support for feminism and feminist organizations that occurred from 1970 to 1974 have now leveled off, Poole and Zeigler anticipate continuing but slow increases in the future. One or both of the women's issues of abortion and equal rights did prove to be highly salient to employed women and housewives in 1980. And attitudes toward the women's movement had much greater impact upon candidate evaluations in 1980 than in past elections. Only one cautionary lesson emerges: Attempts to register and mobilize women voters may only serve to reduce the gender gap, because female nonvoters tend to take more conservative positions than do male nonvoters.

Partisans of the two major parties too will receive conflicting messages. On the one hand, employed educated women are not firmly allied with either party; on the other, women voters present an Arrow's paradox to the would-be strategist. Even the now well-accepted generalizations that women voters are more compassionate in domestic policy and more peace-oriented are challenged by findings presented here.

And for scholars who, realistically, will comprise the major audience for this book, Poole and Zeigler provide the currently most comprehensive and up-to-date analysis of ISPR data broken down by gender. Although the study is more helpful in presenting attitudinal patterns rather than providing an elegant explanation of inter- and intra-gender differences, the building blocks are there for others of a more theoretical bent. Further, the authors predict that despite short-term trends to the contrary, American politics is on a leftward drift led by an electorate increasingly dominated by liberal working women and female officeholders. This assertion is convincingly supported by their data and thus bears careful monitoring.

JANET K. BOLES

Marquette University

Contemporary Public Policy Perspectives and Black Americans: Issues in an Era of Retrenchment Politics. Edited by Mitchell F. Rice and Woodrow Jones, Jr. (Westport, Conn.: Greenwood Press, 1984. Pp. xii + 213. $29.95.)

This is a collection of 11 original essays and an introduction by the editors that examines selected public policy issue areas as they relate to black Americans. The theme of retrenchment is that the black American community faces a "new crisis" because policymakers assume today "that prior programmatic efforts to help the black community were unsuccessful and costly, so retrenchment from federal authority and responsibility is justifiable" (p. 4). The volume should be especially useful to those interested in the benefits and burdens of Reagan administration policies on black Americans.

No theory of racial public policy is set forth, although the editors attempt to give coherence to the volume by outlining in the introduction what they call a "policy analysis approach" to the study of public policy. However, their exposition of this approach is too brief and superficial to be useful, and it is not employed in any of the following essays.

William Nelson and his colleagues examine the effects of Reagan policies on the cities and find that the "overall consequences of these policies are to depress the quality of life for urban blacks and other minority groups" (p. 31). Lenneal Henderson and Michael Preston analyze black public-sector employment in the light of Reagan policies, and in a separate contribution Henderson discusses the negative and positive impacts of recent changes in federal policy toward the small business sector in the black community. Huey Perry's subject is black participation in the formulation and implementation of national energy policy during the Carter admininstration. Patricia Shields focuses on how military policy affects blacks, concluding that "the military is not designed to be an employer of last resort, but it does seem to take on that role. In doing so, it masks the problem that the civilian labor market has absorbing young semi-skilled blacks" (p. 102). Mark Rosentraub and Karen Harlow's examination of black community-police relationships shows that the black community, in spite of the reforms of the 1960s and 1970s, continues to suffer from ineffective police services.

J. Owens Smith, in an excellent analysis of affirmative action from both jurisprudential and policy perspectives, argues that "judicial sanctioning of the claim of reverse discrimination constitutes a double standard of judicial review. The operative effect of such public policy is to deprive blacks of their liberty interest rights to acquire a just share of government supported benefits, that is, jobs, education and fringe benefits" (p. 123). Rice and Jones review the contemporary "crisis" in black health care and in separate chapters by economists James Stewart and Louis Green it is concluded that there is little prospect of any real progress in narrowing the gap in economic status between black and white America without a comprehensive redistribution plan of state intervention in the operations of the market economy. The final essay by Mylon Winn presents an ethical perspective on how black administrators can address the conflicting expectations they may encounter from their professional colleagues and the black community as they perform their official duties.

The overall quality of the individual contributions is high, and most chapters are well documented. The work would have been more substantial had the editors developed a more rigorous conceptual framework for the papers and prepared a concluding chapter to put the contributions in perspective. Nevertheless, students and professionals in American government, urban politics, public administration and policy, and black politics should find the volume useful.

ROBERT C. SMITH

Howard University

Law Enforcement Planning: The Limits of an Economic Analysis. By Jeffrey Leigh Sedgwick. (Westport, Conn.: Greenwood Press, 1984. Pp. xix + 198. $29.95.)

At first glance we expected that this book would focus on police and that it would provide an extended critique of the application of welfare economics to law enforcement. On both counts the title is misleading. In actuality, Sedgwick is concerned with how policymakers should allocate public resources for crime control in general. Furthermore, he spends most of his time describing and not criticizing the welfare economics model.

In commendably clear prose the author describes how welfare economics might contribute to sound criminal justice policy. According to this view the state tries to minimize the costs of crime to society. Criminals are assumed to violate the law because the net benefits of illegal activity exceed those of legitimate activity. Crime policy, then, should be directed at manipulating the structure of incentives by increasing the cost of crime (to the criminal) and/or by increasing the benefits of legitimate activity. The former is accomplished by increasing the certainty and/or severity of punishment. Sedgwick dismisses the latter because it would have unintended—and undesirable—consequences.

Like other contemporary writers in criminal justice (the most notable example is James Q. Wilson), Sedgwick rejects the sociological approach to public policy. According to Sedgwick, this concern with root causes is impractical, and it violates norms of constitutionality. It strikes us that this conclusion is too simplistic. First, the welfare economic model is built on a very explicit assumption regarding root causes. Specifically, the approach is grounded in the idea that offenders are rationally calculating individuals. To reject a concern with root causes while simultaneously assuming a single cause for rather complex and varied behavior strikes us as unsound. Second, efforts to identify causes are not automatically irrelevant to policymaking. It strikes us that the study of proximate causes, for example, could carry substantial policy payoffs. The sociological and economic approaches to crime control are not mutually exclusive. As Norval Morris argues in his book *Madness and the Criminal Law* (University of Chicago Press, 1982), they can be applied in proximity if not simultaneously in some circumstances and to some ends.

Much of Sedgwick's book is devoted to a description of the welfare economics model. The author does, however, address three principal shortcomings of this policymaking perspective. First, there are practical limitations related to the measurement of the public's demand for state action or law enforcement. Sedgwick observes that survey instruments designed to measure the public's willingness to pay are flawed. Second, the welfare economic approach is theoretically limited insofar as it places exclusive emphasis on citizen preference for crime control. Sedgwick maintains that citizen preference is too volatile to form the basis of policy and that it may at times even run counter to principles of due process, fairness, and equity. Additionally, the author agrees that the conception of punishment implicit in the welfare economic approach is unduly restricted. In his introduction Sedgwick quotes Walter Bern's argument that self-interest alone is not sufficient to maintain public law and order, and he returns to this point, albeit briefly, at the end when he states that punishment must be based on retributive as well as deterrent principles.

We share the author's basic criticisms of the welfare economic model and agree that such an approach is insufficient for public policy. We completed our study of the book, however, with the wish that he had concentrated more on the critique and offered a more comprehensive analysis of the limitations that he notes. An elaboration along these lines could focus on several issues and points. These include the willingness to pay measure and the criteria, other than citizen demand, upon which policy choices should be based.

Reviewing Sedgwick's discussion of the willingness to pay measure for citizen demand, it struck us that related surveys are not likely to yield meaningful results. First, they are likely to tap into "non-attitudes." Second, it is possible that such a willingness is simply (or at least partially) a function of income. Demand functions derived from such measures, then, will be inequitable according to many standards and not more democratic, as Sedgwick appears to claim, than the principle of one man, one vote.

In the author's brief discussion of the limits of the economic model, he argues that public policy preferences must be tempered by representative government, and he urges policymakers to exercise sound judgment. We don't quarrel with that, but think that more interesting questions revolve around the substance of sound judgment and the criteria that could be used to measure it. If we are concerned with practical applications such as crime control, then surely these operational issues need to be addressed.

The synoptic problem-solving approach that Sedgwick describes as the welfare economics model requires an enormous amount of information, information that is neither available now nor likely to be available in the future. Extant research on crime reduction effects of added police manpower, for example, is not as uniformly supportive of the deterrent hypothesis as Sedgwick's short list of citations would suggest. Even if it could be shown that, for example, higher arrest rates are causally related to crime or victimization measures, we do not know how to increase arrest rates. Sedgwick assumes that both the certainty and severity of punishment can be affected by law enforcement and appears, in the process, to put the prescriptive cart before the analytic horse as some of the evidence not only does not support but contradicts these assumptions.

On the whole this book makes some contributions, chiefly Sedgwick's succinct description of the welfare economics model. The work would have been stronger had the author concentrated on the critique that he promised.

SUSETTE M. TALARICO

ROBERT E. WORDEN

University of Georgia

The Crisis of Growth Politics: Cleveland, Kucinich, and the Challenge of Urban Populism. By Todd Swanstrom. (Philadelphia: Temple University Press, 1985. Pp. xvii + 307. $24.95.)

Does space exist for a viable redistributive politics in America's central cities? Or, as Paul Peterson has argued in *City Limits* (University of Chicago Press, 1981), are cities the captive of market forces that permit them to pursue redistributive policies only at the price of lost private investment? Todd Swanstrom addresses these questions in a theoretically ambitious account of the administration of Cleveland's self-styled "urban populist" mayor Dennis Kucinich. He examines the Kucinich experience in terms of the logic of "growth politics," understood as the effort of governments to "entice mobile wealth to enter their boundaries" (p. 4).

Swanstrom seeks to give theoretical and historical meaning to Kucinich's belief that the manipulation of a corporate elite, not objective economic pressures, causes cities to succumb to conservative growth politics. He rejects the pluralist model of urban politics for disregarding how investment decisions constrain local politics. As well, he criticizes Peterson's "market model" for failing to recognize that investors and cities can and do exercise "political discretion" in responding to one another. A three-chapter historical prelude to his analysis of the Kucinich experience shows that growth politics in Cleveland has a long history. Of particular interest here is how the rise of the service economy has generated a need for downtown office space, bringing to prominence a new business elite desirous of government intervention to create efficient downtown infrastructure.

Kucinich's brief term (1977 to 1979) is examined under three headings: the politics of tax abatement, the machinations surrounding the city's default, and the conflict over community development. In examining the first, Swanstrom argues, contrary to the assumption of the market model, that the city had the political discretion to abandon tax abatements for downtown office construction. Kucinich proved that corporate offices would be built even without local tax subsidies. In the case of the city's default, brought about when local banks refused to rollover short-term loans after unsuccessfully pressuring Kucinich to sell the city's electric company, it was the banks who exercised political discretion, Swanstrom maintains. In the chapter on community development conflict the author explores the paradox that Kucinich's Alinsky-style confrontation politics worked against his ability to gain the support of Alinsky-style neighborhood organizations. Besides a provocative if somewhat complicated conclusion in which Swanstrom addresses himself to the space for redistributive politics, there is also an Afterword in which he assesses Cleveland's proclaimed comeback since Kucinich's defeat.

The Crisis of Growth Politics is a valuable, thought-provoking contribution to the urban politics literature for its concrete historical examination of the interplay between "private" investment decisions and local politics. Against the pluralist model it demonstrates what others have argued abstractly, but few have shown concretely: that local policy choices are powerfully influenced by investor decisions. In arguing, against the market model, that investment decisions under certain circumstances are motivated by political goals rather than market pressures the book raises more questions than it answers, however. Part of the problem is the absence of a clear and defensible distinction between political behavior and market behavior; without this it is hard to determine whether, as in the banks' refusal to rollover the city's notes, political discretion was exercised. Finally, Swanstrom makes a strong if nonetheless speculative case for the ability of headquarter cities—those already enjoying an office-construction boom—to substitute redistributive politics for growth politics. Accordingly, his book will be of interest to practitioners as well as students of urban politics. The difficulty of the text may, however, limit its readership.

RICHARD E. FOGLESONG

Rollins College

Equality in America: The View frm the Top. By Sidney Verba and Gary R. Orren. (Cambridge, Mass.: Harvard University Press, 1985. Pp. x + 334. $25.00, cloth; $12.50, paper.)

This book is exceptional for clear presentations of the various meanings and implications of equality and of the tradeoffs and priorities with respect to those meanings and implications. The authors' discussion is helpfully organized in terms of six dimensions: equality of opportunity, equality of results, political equality, economic equality, individual equality, and group equality. Tradeoffs and priorities can thus be considered in the context of common policy dilemmas and mixtures of opportunity/results, political/economic equality, and individual/group indulgences and deprivations.

The heart of *Equality in America* is the analysis of responses from 10 groups of American leaders: three "establishment" groups, business, labor, and farm; three "challenging" groups, blacks, feminists, and youth; and four of what might be called "system" groups (although the authors do not call them that), Democratic party leaders, Republican party leaders, media, and intellectuals. The ambitious research design called for questionnaire responses from a sample of 600 of each group. A 56% response yielded 2,762 questionnaires of about 200 items.

For persons generally familiar with American elite opinions and attitudes there are no surprises in this book. There are confirmations of these significant generalizations:

1. Values are more important for creating and maintaining attitudes than are demographics and economics. The differences among American leaders stem not so much from differences in wealth, education, social background or occupation as from the different priorities and meanings given to the various aspects of equality.

2. Equality of opportunity is the central meaning of equality in America. There is a mammoth consensus on it. No other meaning of equality is highly favored—even among the "challenge" groups, blacks and feminists, only about one-quarter subscribe to notions of equality of result. The standard American view is that the race should be fair, not that everyone should win.

3. The debate is over whether equality of opportunity has been achieved in contemporary America and if not what more should be done to achieve it. Here there is nearly as much dissensus as there is consensus on the equality-of-opportunity ideal.

4. Political parties, denigrated as they have been for 40 years in lay and professional commentaries, are still the principal agencies around which attitudes relating to equality cluster. The parties are sharply polarized in their platforms, in their constituent groups, and in the attitudes of their leaders; this polarization is most pronounced with respect to economic equality, but it is strong as well in race and gender equality.

Equality in America has one serious flaw: It is a new book presenting old data. The research for the project was done in 1976 and 1977. The book has just been published. Why the nine year delay between data collection and publication? We needed these findings six or seven years ago. We still need them no doubt, but much less urgently.

One consequence of this delay is that the authors' comments that relate their findings to current mass and elite attitudes during the Reagan administration must be discounted. If values are central—as the authors claim and as I agree—and if values change, even incrementally with governmental programs and intellectual fashions then it is probable that the attitudes found in 1976 are to some degree different from those of 1981 through 1985. Chapter 7, for example, describes the attitudinal differences of Democratic and Republican party leaders in 1976 on six sets of equality issues and reveals wide polarization on "gender" and "quota" issues especially. There is some evidence that in recent years Democratic party leaders have adopted more conservative or centrist views on gender and affirmative action policies; we would like more up-to-date findings if we are to link leaders' attitudes with contemporary policymaking.

A small final point. While awaiting publication, the authors could have taken note of a parallel and in some ways very complementary work, Karl Lamb's *The Guardians* (Norton 1982), an investigation of the political values of 117 American leaders personally interviewed by Lamb in 1977 and 1978. Perhaps survey researchers do not owe us extensive reviews of the literature—for, after all, there is so much of it and we may be expected to be generally familiar with contemporary poll data—but should not elite studies acknowledge similar work and give us some comparative commentary?

BERNARD HENNESSY

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Congressional Budgeting: Politics, Process, and Power. Edited by W. Thomas Wander et al. (Baltimore: Johns Hopkins University Press, 1984. Pp. xi + 250. $27.50, cloth; $11.95, paper.)

This anthology of collected research on the congressional budget process presents a theoretical perspective for further research and raises interesting questions regarding the usefulness of the 1974 reform for the first time. Much like the process itself, however, the contribution of each chapter to the theoretical perspective varies. Nevertheless, each chapter evaluates the impact of reform from a decade-long perspective.

The first two chapters develop an analytical perspective for some of the research in subsequent chapters. Borrowing from Herbert Simon and other organizational theorists, the authors present the initial pressures for reform and subsequent congressional budgeting actions as consequences of both internal and external variables not considered as comprehensively previously. Although the presentation of their theoretical perspective is not particularly novel to congressional literature, it represents a needed development for those engaged in congressional budget research.

The second collection of research examines the institutional impacts and changes generated by the reform within each chamber, between chambers, and between the executive and legislature. The primary contributions of these essays are twofold. First, the longitudinal perspective overcomes the time constraints of most prior research. The comparisons of the pre- and post-1980 experiences provide important similarities and differences in the operation of the budget process. Second, the partisan change in the Senate after the 1980 elections has had considerable impact on the effectiveness of the Senate Budget Committee in the Senate and with the House committee in conference. These changes and the reactions by the House have made the process more divisive than during the first five years of the reform.

Despite these contributions, this section of the text has several weaknesses. The theoretical perspective presented in the first chapter is not uniformly applied by each researchers to his/her particular research question. Second, several chapters tend to abandon theoretical development altogether. As a consequence these chapters are susceptible to criticisms of the prior budget literature: idiographic analyses and simplistic presentations of data and variable relationships.

The final three chapters are assessments of the overall impact of the 1974 reform. For the first time, the process is considered a failure by all three authors. The process has become complex and puzzling with a "never-ending series of confrontations on budget figures, the result being constant making, remaking, and unmaking of decisions" (p. 168). The needs for budget centralization and some fiscal constraint have been in direct confrontation with the decentralized nature of the legislature and the proclivity of members to increase spending. Each author makes suggestions for possible reform to correct the most apparent weaknesses. However, the key to further reform is not specific suggestions, but rather as Kenneth Shepsle notes, "one that takes legislators as they are, [and] understands how self-interest works. . . . The current budget process is little more than 'voodoo politics'" (p. 217).

This is a timely text for those seeking to understand the current budgetary predicament. It begins to realistically evaluate the process and make meaningful suggestions for change. The theoretical perspective of the process, while in need of further refinement, is an initial step for those conducting research in this area.

JOHN W. RANDALL, JR.

The University of Alabama in Huntsville

City Managers and School Superintendents: Response to Community Conflict. By Harmon Zeigler et al. (New York: Praeger Publishers, 1985. Pp. xv + 198. $32.95.)

Although comparisons are made between public school superintendents and city managers, this book is mainly about superintendents. So extensive is the concern with them that the book may be of greater interest to educational administrators than to political scientists. As the authors state openly, the real aim of the book is to resolve the apparent contradiction between claims that superintendents dominate educational decision making and claims that superintendents are beleaguered.

Among the major findings are the following. School superintendents spend less time resolving conflict with their legislative bodies and less time generally managing conflict than do city managers. Superintendents appear to face a less conflict-ridden environment. School boards operate with a higher degree of consensus than do city councils. Further, superintendents report low levels of disagreement among the public, and when the school public becomes involved in conflicts, it is usually as individuals rather than as group members. Superintendents also seem to face less administrative and interorganizational conflict than do city managers.

Is it the case that school superintendents feel unbeleaguered? Not necessarily. Although the authors don't have survey data to compare contemporary with earlier times, the historical perspective, they conclude, suggests that finances, collective bargaining, and federal intervention create more problems now than earlier. Significantly, school superintendents report these as problem areas more frequently than do city managers.

Relying mainly on survey data, the authors encounter problems of interpretation. Respondent perceptions are very much a product of underlying predispositions and expectations, but these are probed in no depth. What does it mean, then, that superintendents are less likely than city managers to view conflict management as an essential part of their jobs? The answer is not clear. Superintendents dislike conflict, but the authors find that situational pressures may override predispositions. When superintendents describe how they actually anticipate and regulate conflict, they are every bit as likely as city managers to talk about keeping their political ears to the ground, bargaining, compromising, and working out political forms of collaboration. If they dominate their lay boards, is it because they successfully assert professional authority and expertise or because they are better politicians than their board members? Maybe the information advantage they hold is not restricted to technical information but includes political information as well. If school superintendents are rarely rebuffed by their boards, perhaps it is in part because they are politically astute. The authors appear equivocal about the basic force in superintendent behavior. Sometimes they stress the training of superintendents and their immersion in reform doctrine about technical expertise. At other times they freely acknowledge that school administrators treat their jobs as highly political.

Overall this is a slender volume, only about 150 pages of text. There is no shortage of quantitative data, from extensive reporting of survey responses to governmental data on school finances. Yet the texture of school politics is largely absent, perhaps because the authors believe that case studies inevitably focus on conflict and on the atypical. Despite this view, the nondecision process receives little illumination. One supposes that in a world of routine, bureaucratic decision rules are important, but such rules are not identified. Except for large executive budgets, the authors don't tell us what superintendent domination of decision making means in terms of policy bias and educational practice. So, although there is much useful information in this volume, much is also unexplained. Readers should also remember that the survey findings come from selected suburban jurisdictions in the Chicago and San Francisco areas. As with most social science research, this book makes a case for further study.

CLARENCE N. STONE

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Comparative and Other Area Studies

The State and Revolution in Iran. By Hossein Bashiriyeh. (New York: St. Martin's Press, 1984. Pp. 1 + 202. $27.50.)

The political and social changes in revolutionary Iran have made headlines and inflamed the rhetoric of policymakers in the United States as well as other countries. Although there are fragmented studies on Iran's politics, the bulk of available material deals with the Iraq-Iran War and is based mainly on media reporting or government officials' statements. In general, revolutionary Iran studies are poor in both quality of analysis and scope of research and methods. The impairment of good scholarly work on Iran is the product of the Iranian government's negative attitudes toward empirical research, of the absence of data, and of the fluidity of events that defies conceptualization and the formation of analytical research.

Hossein Bashiriyeh's *The State and Revolution in Iran* is an outstanding scholarly work. It was conceived out of field research conducted in 1980 for the author's Ph.D. dissertation. Aided by his native language and understanding of Iran's society, the author was able to gather considerable data from his own personal observation, contact with government officials, and utilization of original Iranian documents not available outside Iran.

Bashiriyeh seeks to explain the causes of the revolution and the phases through which it has gone, viewing it as a total social change facilitated by internal contradiction between the state's behavior and social classes' expectations. This conflict was not born of the social change introduced by the Shah in the 1960s and 1970s as popularly perceived by Western scholars, but rather evolved since the establishment of the Iranian state at the turn of the twentieth century. The author traces class conflict in Iran to the Constitutional Revolution of 1905, when new social and economic classes emerged on a political scene affected by the monarch's determination to restructure social and political powers throughout the society.

Of all the social classes, the Ulama class played a most important role in Iranian politics. Reaction in early 1900 to Western penetration into a traditional system gave members of the Ulama class "a position of power and they emerged as the proponents of the rising indigenous nationalism which was expressed in terms of Islam. Thus, the increasing power and opposition of the Ulama were more functions of rising nationalism in the face of Western imperialism than the imperatives of Shiite political theory" (p. 9).

The Ulama found a continuing political base for their power in the bazaar. Merchants in the bazaar were connected to the Ulama through the payment of religious taxes and financing religious schools and mosques. Some of the Ulama even "taught and resided in nearby schools and sometimes also owned shops in the bazaar" (p. 13). In chapter 4, the author argues that the social changes introduced by the Shah in the 1960s produced new economic classes of neobourgeoisie and merchants whose vehement opposition to the regime was "influenced by the continuously intervening" government intent on regulating the economy. For different reasons, they developed a holy alliance with the Ulama and undermined the stability of the regime. In the face of the growing opposition, the Shah resorted to political repression and consolidation of an authoritarian regime.

Islamic ideology is discussed in historical perspective in chapter 3. From the outset of the Ulama's political involvement, two schools of thought emerged. The dominant group among the Ulama included those who advocated constitutionalism as a means to limit the king's power and to defend Islam against Western influence; only a minority objected to constitutionalism and called for the adoption of the Shariat as a limit on the power of the Shah.

As the intellectual interpretation of the role of religion increased in the 1960s, some Ulama such as Ayatollah Khomeini began to shift their position from constitutionalism to Islam (p. 60), and he came to believe that Islam and nationalism were under great danger from Western imperialism. Rethinking the relationship between temporal and religious authority, Khomeini revived the Shiite political doctrine of Imamate in which the Imam or his deputy would be authorized by the divine power to rule his community of believers.

The most useful tool for understanding the present Iran's politics is chapter 6, which deals with the construction of the revolution. Bashiriyeh details the activities and ideologies of the various political parties, factions, and groups that emerged during and after the revolution, presenting an admirable compilation.

The State and Revolution in Iran is a valuable contribution to the study of Iran's politics and will be one of the outstanding studies of the system.

ABDUL H. RAOOF

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Ethnic Groups and the State. Edited by Paul R. Brass. (Totowa, N.J.: Barnes & Noble Books, 1985. Pp. 341. $27.50.)

Much of the literature on ethnicity and the state has focused on the question of how ethnic groups and classes interact and affect the political integration of society and hence the political stability of the state. Originating in the Seminar on Comparative Studies in Ethnicity and Nationality at the University of Washington and edited by Paul R. Brass, professor of political science and South Asian studies there, this collection of essays examines the other side of the equation. It raises the question of how the state by way of its ideological assumptions and public policies affects the identity of ethnic groups. Although for a full discussion of ethnicity and the state both questions need to be raised, this outstanding volume should be noted for rectifying our perspective.

Brass himself introduces the collection with an important theoretical statement on the issue of ethnicity and the state. He is followed by Crawford Young's stimulating and wide-ranging essay on ethnicity and the African state. Other fine efforts follow, including Kemal Karpat on ethnicity in the Ottoman empire; David Paul on Slovak nationalism in the Hungarian state; Paul V. Warwick and Leonard J. Cohen on the Yugoslav experience; Davydd J. Greenwood on Castilian, Basque, and Andalusian ethnicity; Maureen Covell on Belgium; Heribert Adam on South Africa; and Alvin J. Ziont on the United State's policies toward Indian tribal autonomy and separatism.

Brass suggests an approach to the problem that he calls elite competition theory. This is an approach that, unlike the Marxists, does not assume a priori that the state is an expression of class relations and that ethnicity can be reduced to structural factors; nor like the cultural pluralist school of Furnivall, M. G. Smith, and Kuper does it assume primordiality and unchangeability of ethnicity; and unlike the interest group approach it definitely does not view the state as a mere passive arena in which interest groups clash and resolve their differences.

Brass suggests (together with some neo-Marxists) that the state can be relatively autonomous of social forces and that it can play an active role in the shaping of ethnic identity. He also proposes that ethnic groups not be viewed as homogenous and unchanging entities, but as heterogeneous and dynamic coalitions each led by its own elites. Such a perspective on the one hand allows him to view the state as an independent factor in the shaping of ethnicity, and on the other hand, to view ethnicity as changeable and malleable.

Brass's views are borne out in the seminal essay by Crawford Young on Africa. There an alien state system was imported by conquest and trade, and it had multifarious effects on ethnic identity. Young suggests that three major effects can be discerned. By labeling and classifying groups into disjoint ethnic categories, the state induced the very reality it had named. By creating a territorial administrative system, it gave ethnicity a dimension that it did not always have in many places in Africa. By providing unequal provisions for opportunities and social promotion among various groups, the state affected not only ethnic identity but also the shape of ethnic conflict. Indeed, similar results are reported by all the other authors.

Although Brass and his colleagues—most notably Young, Karpat, and Adam—stress the role of the state in shaping ethnicity, they also recognize that it is not the only such force. An incipient capitalist economy, the party system, and the system of status stratification also play their role as independent sources of values and inequalities. These dimensions as well as the various cultural and psychological predispositions such as the need for achievement and power have a bearing on the identity of ethnic groups and on their relative positions in the stratification system. For a complete discussion—something that this volume does not claim for itself—such forces and their effects on ethnicity would have to be examined.

ROBERT MELSON

Purdue University

The State Society Struggle: Zaire in Comparative Perspective. By Thomas Callaghy. (New York: Columbia University Press, 1984. Pp. xviii + 515. $45.00.)

There are many virtues in this big book on Zaire, including its "old fashioned" analytic and traditional approach and its often pithy judgments. But there are drawbacks too—some a product of (or intertwined with) the virtues. The most obvious problems are sometimes overwrought theory, size, and an ironic abruptness on certain issues and lack of consideration of others.

Callaghy's purpose is to unravel an often ignored and/or assumed irrelevant aspect of contemporary political life—the normative and philosophical underpinnings of organized public affairs. In this case the focus is on the development of the state form. The author develops this analysis on the assumption that the new state mode of an entity like Zaire requires a period of conscious effort at creating a state essence (idea and form). In addressing this problem, a very rich and often redundant comparative analysis of *caudillismo* in Latin America, absolutism in France, and mobilizational authoritarianism in Zaire is presented. This is done with deftness in places and provides a genuine sense of this task in African states to create that ideal form for postcolonial governance and identity. The conclusion here is that in Zaire, as in the France of Louis XIV, the leader takes serious license with the rights of the body politic in forging ahead. Callaghy cites, often too briefly, the full range of well-known antidemocratic and corrupt practices of the Mobutu regime—departicipation, personal financial aggrandisement, ironic prolongation of and dependency upon colonial administrative structures, and complete domination of the policy process. These difficulties in Zaire are skillfully compared with the historical development of political processes in Latin America and France. The historical data are rich, and the similarities obvious for three different nations at a time when they were at an early stage of state identity—that is, successfully merging a host of subnational units into relatively large and contrived public entities. This kind of cross-cultural analysis is most welcome.

A perhaps inevitable problem, but one of some consequence, lies in the focus on state formation. Because the analysis is so heavily concerned with state forms, there is an overconcentration on the development of structures and not enough on how citizens consciously and/or inadvertently affect such structures. In short, this analysis itself is very statist. Its overwrought theory on the Mobutuist structures obscures considerable data on day-to-day citizen actions (or lack of actions) that would broadly strengthen Callaghy's claims about the regime. This is certainly evident when in the last 75 pages he allows us to peek into the collectivities (local areas). Here it is boldly evident that all of his hunches about the problem of penetration for states like Zaire are correct. Despite the best efforts of the administrative center to have its policies and administrative structure to be of force in the periphery, this hardly happens. A case in point is especially the *pays d'etat* (local areas administered by traditional chiefs). These local chiefs successfully ignore, harass, or otherwise circumvent dicta with which they disagree—sometimes with great flourish. Of course this raises still another dimension (which Callaghy does not tackle) of the disjuncture between the claim of success for regime institutions and lack of success of local collectivities. Indeed this begs the question of whether the selective cases of Kivu and Bas-Zaire are representative.

Callaghy does not elaborate sufficiently on some issues. It is not enough to merely mention such powerful issues as churches, refugees, and ethnic groups, or to selectively look at one or another region. Indeed, it is here that the irony of size emerges. Clearly it would have been a "larger" book if some of these issues had been taken up more fully. Yet when by the author's account refugees are numerous and cause considerable problems on two or three borders, it would seem that this should be taken up at some length, and it is not. Or, what about the complex dimensions of ethnicity and/or linkages between the regime and international capital? All these are mentioned, but not fully explored for their import upon the character and functions of the regime. On the other hand, if a careful effort had been made to acquire aggregate data on some aspects of all the regions, the size of the volume may have decreased, and the foundation for conclusions may have been more solid.

Criticisms aside, this is a fine piece on one side of the Zairian picture. Callaghy is clearly correct that it is instructive to analyze the philosophical and accompanying structural parameters of the contemporary state in Zaire and Africa. Although here and there the somewhat Baroque writing style leads to redundancies, in other instances it literally rings with a poetic quality. Perhaps it is just as well, because much of everyday public life in Zaire has a kind of ironic character. Callaghy is surely right when he says, "theoretical absolutism has been so grandiosely promulgated because the reality is often so shallow" (p. 143).

Minion K. C. Morrison

Syracuse University

Rethinking the Soviet Experience: Politics and History since 1917. By Stephen F. Cohen. (New York: Oxford University Press, 1985. Pp. xiii + 215. $17.95.)

A book arguing for reevaluation of American stereotypes of the Soviet Union is invaluable, especially at this juncture in Soviet-American relations. Stephen F. Cohen's collection of five essays, written in the last eight years and revised in 1984, is a copiously noted, scholarly work of historical revisionism. Cohen focuses in the first two chapters on the concept of totalitarianism and the method of interpreting Soviet history according to the "continuity thesis" (p. 40) and the distorted accounts that result therefrom. His illustrations of McCarthy-era persecution of such pioneers as John N. Hazard reveal forces girding the 20-year consensus on empirically inaccurate and logically inconsistent methodology. In chapters 3 through 5, Cohen revises interpretations of three major questions of Sovietology. Bukharin's role and the New Economic Policy (NEP, 1921-1928) are evidence in Cohen's third chapter of viable alternatives to Stalinism arising within Bolshevism and surviving to the present. Cohen views Stalin's years as an aberration in Soviet history that also gained lasting adherents visible today among both elites and masses. In chapter 5, Cohen argues that competition between reformism and conservatism is a constant in Soviet history that belies the assumptions of totalitarian continuity.

Although Cohen's generalizations about the stifling effects of the development of and adherence to one model of Soviet politics and history are irreproachable, he does commit some of the fallacies on which he bases his criticism of others. In chapter 1, Cohen quotes phrases extensively to illustrate the dominance of totalitarianism as the 1950s and 1960s framework for analysis. Although there still are adherents to this model (as, Jeane Kirkpatrick), Cohen overstates the interpretive conformity to bolster his argument. The first three subsections of chapter 1 document Cohen's charge of conformity to a totalitarian-straight line model. The last subsection considers changes starting in the 1970s (p. 27), but cites literature first published in the 1960s (see, especially, notes 88-90). Exclusion of works on international communism and economic reforms omits many 1960s works that rebut the totalitarian model. Bernard S. Morris, quoted on page 18 in support of Cohen's view, taught at Indiana University varying interpretations of Soviet history and politics written and compiled in the period Cohen decries for its lack of theoretical diversity.

Cohen's view of the NEP as evidence of a real alternative to Stalin's forced collectivization and industrialization and as precursor to contemporary economic reforms (p. 141) suggests the "Whig tradition in Soviet studies" (p. 51). Arguing that contemporary attempts to reform the Soviet economy (and even further afield, current PRC reforms, p. 153) derive specifically from Bukharin's advocacy of the NEP, Cohen mimics the Whig straight-line method of ignoring events of intervening decades. The argument also ignores the interactive effects of East European economic reforms of three decades.

Similarly, although decrying the oversimplification of treating Soviet politics as monolithic in the first two chapters, by the last chapter Cohen reduces policy debates in the USSR to a universal and timeless conflict between reformism and conservatism. On the level of broad-brush historical generalization the interpretation holds, but it tells very little about changes over time in Soviet politics. Although promoting revisionism in the first chapters, Cohen omits discussion of specific policies in the last chapter that would illustrate the benefits of the advocated revisions. Cohen criticizes the continuity thesis but does little to illustrate discontinuities in later chapters.

Each of Cohen's essays stands on its own logic, but the five display repetitions and contradictions. Cohen's emphasis on conceptualization and interpretation rather than on historical description may limit his audience to those already schooled in Soviet studies. Regardless, the collection promises to spark controversy and promote much needed scholarly revision. Cohen's book is required reading for those in Soviet studies.

CONSTANCE H. COLE

Mount Holyoke College

Electoral Change in Advanced Industrial Democracies: Realignment or Delightment? Edited by Russel J. Dalton et al. (Princeton, N.J.: Princeton University Press, 1984. Pp. xvi + 504. $55.00, cloth; $14.50, paper.)

At the heart of this well-orchestrated study of electoral change in advanced, industrial democracies there stands the impact of new generations of voters and their concerns upon, for the most part, well-established party systems and social cleavages. The old cleavages of the process of industrialization, alas, are waning; church, class, trade union, and other partisan loyalties of the voters are weakening; and the very function of political parties, at least among resourceful voters in modern democracies, is being eroded by the emerging alternatives of political action and organization, as well as by the seemingly ubiquitous signs of electoral dealignment. The excellent country studies presented here just about all bear out the thesis of considerable dealignment, even in France and Canada where the editors insist on seeing continued "stable" alignments, and in the United States, West Germany, Japan, and Italy, where processes or realignment are said to be emerging against the background of dealignment but have not really managed to establish stable new patterns.

The reasons why they have not are many, beginning with problems of institutional lag and behavioral inertia, which keep the established parties and party systems still remarkably immune to the secular erosion of their social bases. There are the ever-increasing generational, "postmaterialist" cohorts and their New Politics, to speak with Ronald Ingelhart (p. 68), to be sure, and they in turn have had their influence on the established parties, spawned new parties, and created alternative channels of political action. But in spite of all the changes underneath, the evidence so far of a partisan realignment in the United States and the Federal Republic has only been a "parliamentary realignment" (p. 98), and in Italy and Japan only a relative decline of their respective dominant parties. Perhaps too, the postmaterialist forces and issue concerns have been consistently underestimated by public opinion studies that fail to distinguish, for example, between the issue of unemployment as an Old Politics issue and the New Politics, life-style aspects of youth unemployment, or that similarly lump together diverse value colors of security (defense) and are nearly silent about the potentially realigning impact of the peace movement. Although this team of researchers has not done it, it must be possible to refine the instrument of measurement enough to separate old and new cleavages, for instance regarding abortion (p. 120), rather than having a large pocket of "mixed" values.

The biggest obstacle to the translation of the changes in the electorate into actual partisan realignment or, alternatively, a "withering away of political parties" (p. 454), are obviously these organized parties themselves. The party elites can take up or refuse some of the new issues, split on paper over their internal disagreements, and, most of all, maintain a competitive game of politics that, by electoral laws and other rules of the game, frustrates the coming out of the new groups, and even of new issues, barring major crises. Many of the authors and the editors in this volume acknowledge the crucial importance of these factors of party leadership and structure which, of course, lie beyond the reach of this technically highly accomplished and unified electoral study. Thus, in spite of its seeming perfection, this book directs the reader to go on beyond it to the study of parties and alternative movements themselves in order to understand how and why established parties can turn away the challenge of the new issue conflicts and how the emerging modes of alternative political action fare in this context.

PETER H. MERKL

University of California, Santa Barbara

The Politics of Public Enterprise: Oil and the French State. By Harvey Feigenbaum. (Princeton, N.J.: Princeton University Press, 1985. Pp. xvii + 194. $25.00, cloth; $8.95, paper.)

This book attempts to develop a general analysis of the relationship of the state to society, using public enterprise as the test of a state's "power." The author has chosen his case study—the nationalized oil industry in France—carefully, pointing out that the state's ability to establish a public enterprise in the putatively profitable oil industry is a good indicator of the state's "power," and quite distinct from other nationalizations of weak or unprofitable sectors. Likewise, he has chosen to examine the extent to which the French state lives up to its popular image of archetypal administrative autonomy and authoritative state control over the economy. His conclusion is that the French state's "power" is largely illusory. Despite the legal realities of public ownership of the two oil firms—or to some extent precisely because of the complex and contradictory mandates imposed by the state—this industry has not acted in ways consistent with the "national interest." Indeed, according to Feigenbaum, their behavior is scarcely distinguishable from that of private (foreign) companies.

The author examines the circumstances surrounding the creation of the two corporations: the Compagnie Francaise des Petroles (CFP), created in 1923 and 35% publicly owned, and the Entreprise de Recherches et d'Activites Petrolieres (Elf-ERAP), created in 1945 and 100% publicly owned. He finds that exogenous factors rather than an overwhelming state power precipitated state intervention, although, as he points out, the French environment—thanks to political culture, elite attitudes, and market conditions—was not averse to the notion of state intervention. Further, Feigenbaum contends that the choice of public corporations as the instrument of that intervention was a product of the political conjuncture rather than any explicit theory.

The reasons for the intervention were fairly classic: to prevent the consolidation of a monopoly situation in a vital resource and to protect the national interest, in this case defined as an assured adequate supply of oil at a reasonable price. However, Feigenbaum finds that the specific administrative means used undercut the original goals. Managerial autonomy, decentralization of authority, and a requirement of profit maximization led these public corporations to concentrate on the corporate, rather than the public, aspects of their enterprises and thus to behave in ways scarcely distinguishable from those of other oil companies. Thus, a structural conflict of interest developed between the nationalized firms and the state administration. The loser was the public interest.

Feigenbaum suggests a great many interesting points, but tries to do too much and therefore does not adequately cover any one issue. He could have made a real contribution by discussing in detail the circumstances surrounding the creation of these corporations and the arguments used at the time for this particular form of internal organization. He proposes that the choice of public corporation was a product of the constellation of political forces, but does not explain what those constellations were in 1923 and 1945. Of particular interest would have been some attempt to compare those circumstances with those in 1981 and 1982, when the socialist government of Francois Mitterand embarked on an ambitious program of nationalization followed three years later by a widely noted "privatization" of those enterprises. Unfortunately, except for acknowledging the new government, the book makes no mention of the socialist program.

The book's theoretical conclusions are seriously limited by a failure to define what Feigenbaum means by "state." If a central question in his study was whether France was a "weak" or "strong" state, a clear understanding of what he was examining would seem critical. At a number of points in the book he seems to be confusing state with administration, in which case he seems to be criticizing the administration for failing to control itself in a public corporation. This oversight is particularly surprising given the large theoretical and empirical literature in French on both the state and the administration.

Finally, neither the central question under study—whether state firms in France act differently than private firms—nor the conclusion that they do not is terribly new. Analysts, particularly the French, have been criticizing state firms on precisely this point for 30 years, but perhaps most pointedly since 1981. Specialists on France, therefore, will learn nothing new of significance, but public policy analysts in the United States may benefit from an examination of another nation's experience with public corporations.

RITA MOORE-ROBINSON

The American University

Wage Restraint by Consensus: Britain's Search for an Incomes Policy Agreement, 1965-79. By Warren H. Fishbein. (Boston: Routledge & Kegan Paul, 1984. Pp. vii + 295. $24.95.)

Wage Restraint by Consensus examines the failure of the 1973 social contract that was agreed upon by the Labour party and the Trade Union Congress (TUC) to restrain wage inflation. Fishbein argues that British unions failed to abide by a social contract in which they would restrain wages in return for a voice in economic policy and specific policy concessions from the government because of their voluntarist ideology and the devolution of collective bargaining to the shop floor. Fishbein concludes that the proven ineffectiveness of the social contract leaves no alternative but monetarism as a means to restrain wage inflation.

Fishbein's account of the period from 1974 to 1979 in which the Labour party and the TUC tried to abide by a social contract can be separated into three periods. The first period covers the first 18 months of Labour's administration in which the government upheld its side of the social contract, but the unions did not uphold theirs. The government repealed the Industrial Relations Act, took into public ownership failing industries, and increased pensions. Despite these concessions a wage scramble by the unions ensued led by militant shop stewards. This led to the second phase, from 1975 to 1977, in which British unions did adhere to wage guidelines. Unions adhered to the wage standards because they had widespread public support because of the national economic crisis, and unions were reluctant to test their industrial strength in such depressed market conditions. Ironically, in the first phase of the social contract, concessions from the government failed to elicit the cooperation of the unions. Now in the second period, the unions cooperated with the government despite the failure of the government to "deliver fully on its side of the contract" (p. 223). For example, in 1975 the Labour party's budget suspended a long-standing commitment to full employment, and Labour rescinded planned increases in public expenditures.

Two years of wage restraint created pressures within the unions that finally exploded and brought the social contract experiment to a close. Skilled workers sought to restore differentials that had been squeezed by restraint, and British workers sought to recover the decline in real earnings they had experienced. Beginning in 1977, a third phase of the social contract began that saw militancy return to the shop floor. This new wage scramble made the social contract a dead letter and sealed the fate of the government.

Fishbein is thorough in his coverage of the negotiations over the terms of the social contract between the TUC and Labour government. This material updates the work of Leo Panitch (*Social Democracy and Industrial Militancy,* Cambridge University Press, 1976) and Gerald Dorfman (*Wage Politics in Britain,* Iowa State University Press, 1973). In addition, his penultimate chapter is a fine assessment of the degree to which the government failed to fulfill its obligations in such non-wage areas of the social contract as industrial policy, public expenditures, income redistribution, and industrial democracy. Although Fishbein largely blames the unions for the failure of the social contract, it is also plain that the government was a less than faithful partner. For reasons that are unclear, Fishbein is far more willing to forgive the omissions of the government than those of the unions.

In an otherwise trenchant and tightly argued book, Fishbein's conclusion is an anomaly. There is a "round up the usual suspects" quality to Fishbein's charge that militant shop stewards in the plants sabotaged wage restraint. The role of the shop steward is more complex than simply a fomenter of rank and file grievances as appears here. Moreover, if the case for a social contract is weak in Britain because the union leadership lacks the authority to impose a decline of living standards on the membership and wants to preserve the autonomy of their unions, the case for monetarism is weaker still. Fishbein argues that "in the long run" monetarism will have a less painful effect on the economy than a social contract policy that will eventually crumble (p. 240). But the economic future looks as bleak for Britain today as it did when the Conservatives took office six years ago. All of the social costs of monetarism that the British have endured—plant closings, unprecedented unemployment, urban decay—have been in vain. Monetarism has not delivered on its promise. The only economic truth that holds "in the long run" is what Keynes said lies in store for all of us.

ALAN DRAPER

St. Lawrence University

The Changing Civil Service. By Geoffrey K. Fry. (Winchester, Mass.: Allen & Unwin Publishers, 1985. Pp. 162. $20.00, cloth; $8.95, paper.)

Geoffrey K. Fry has written a short book to discuss the changes, or seeming changes, in the British Civil Service since the publication in 1968 of the Fulton Committee Report, which constitutes the preeminent recent British effort to effect administrative reform of the civil service and ranks in importance with the 1978 U.S. Civil Service Reform Act. Hence, an examination of if and how the Fulton Committee report has changed the administrative process in Great Britain is a worthwhile endeavor.

To accomplish this task Fry begins with a brief chapter, "The Changing Civil Service." After delineating demographic changes in the nonindustrial civil service, he cautions that it would be an error to consider the Fulton Report as "the test of change" because "this interpretation tends to ignore the fact that the important changes in the service were well under way before Fulton" (p. 6). These changes occurred in civil service spheres such as recruitment and training arrangements, the pay system, and union activity. Moreover, Fry claims that the Fulton Committee report's major conceptual deficiency was its scant attention to the doctrine of ministerial responsibility. Thus, the committee "could not properly consider the modern working of the minister-servant relationship" (p. 7).

Fry attempts to rectify this alleged conceptual lacuna in chapter 2, "The Higher Civil Service in the Machinery of Government," by stressing the virtues of a nonpoliticized civil service. He feels "that the civil service has the right to guide governments towards a 'common ground' seems questionable" and affirms that Sir John Hoskyns, Prime Minister Thatcher's former advisor, was correct when he said "that the civil service has no legitimate role in acting as constitutional ballast" (p. 27). Fry then asserts that the major reason governments become "heavily dependent on the higher civil service remains the unwillingness or inability of British political parties to prepare for office" (p. 31). If this is so, and if British political parties are either in a dealignment process or if the entire party system is even undergoing a fundamental decay, then one should not be surprised to see further politicization of the British civil service.

Yet according to Fry, the "first Thatcher government asserted the supremacy of the politician" over the civil service despite "some opposition, even disloyalty" (p. 33). After acknowledging this victory, Fry discusses various topical civil service issues within the framework of the following chapters: 3, "Managerial Issues in the Changing Civil Service"; 4, "The Central Management of the Civil Service Question"; 5, "The Changing Civil Service Pay System"; 6, "The Changing Character of Civil Service Unionism"; and 7, "The Quest for an 'Efficient' Civil Service." Each chapter contains an analysis of the extent to which the Fulton Committee recommendations, which Fry calls a combination of conservatism and radicalism, have been implemented during the 1970s and early 1980s. These analyses are detailed, informative, and presented in painstaking fashion.

But Fry aspires to more than mere descriptive analysis. In the Preface he describes this work as "an interpretive essay of assessment." Although he declines "to treat the Fulton Report as if it were equivalent to the Bible," he claims that he is not hostile to reform, unionization, and reasonable renumeration plans. Hence, "a simple-minded classification of this book as 'anti-Fulton' or 'anti-civil service' would be misguided," as he states in the Preface. Why does Fry feel the need to defend himself at the outset against allegedly "simple-minded" interpretations of his work? Simply because although it may not necessarily be "anti-Fulton" or "anti-civil service," it is appropriate to characterize this book as a fundamentally pro-Thatcherite interpretation of a proper civil service. Fry's ideological proclivity manifests itself most tellingly when he describes the civil service union's strike action against the Thatcher government. At several points he depicts the union activists as virtually mere rabble inasmuch as they were incapable of being "inhibited by traditional middle-class mores about strike action" (p. 134). If these union activists were indeed Marxists, why should one be surprised or appalled if they do not adhere to "traditional middle-class mores" when engaging in conflict?

Fry concludes his essay by explaining why the Thatcher government has "had an advantage over the Fulton Committee" (p. 147). In the middle of this explanation he observes that "Unlike with Fulton, the civil servants could not be so sure this time that their hierarchies and privileges would emerge unscathed" (p. 147). Surely this is an understatement!

Fry has presented an often lively account of issues that many political scientists probably deem much less than intellectually exhilarating. This is indeed laudable. Yet Fry presumes that the reader is familiar with British government and administration. Although the book would probably not be readable for most American undergraduates, it would be informative and useful for graduate students conversant with British politics and interested in comparative administration.

PHILLIP C. WALL

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Competing Equalities: Law and the Backward Classes in India. By Marc Galanter. (Berkeley: University of California Press, 1984. Pp. xxii + 625. $36.50.)

Affirmative action is increasingly under attack. It is often equated with reverse discrimination, which is considered antithetical to our commitments to merit and equality. But formal equality, that is, equal opportunity, for historically oppressed communities in modern democracies often seems inadequate. Is preferential treatment of oppressed communities necessary in the short run to ensure real equality and utilization of merit in the long run? How can the oppressed communities be fairly identified? How is the danger of perpetuating the communal distinctions to be avoided? Should preferential policies emphasize group or individual disabilities?

Marc Galanter examines these questions in this excellent book by analyzing more than 30 years of Indian experience with what he calls "compensatory discrimination" policies. The focus of the book is the role of the higher judiciary (state High Courts and the Supreme Court) in defining and delimiting the contours of the compensatory discrimination policies. The first part of the book provides an introduction to the historical and social structure in India and the development and major features of compensatory discrimination policies. The preferences involve (1) "reserved seats in legislatures, reservation of posts in government service, and reservation of places in academic institutions"; (2) "expenditure or provision of services, e.g., scholarships, grants, loans, land allotments, health care, legal aid"; and (3) "special protections . . . efforts to protect backward classes from being exploited" (p. 43). There are three main beneficiary classes: scheduled castes, formerly called untouchables; scheduled tribes; and other backward classes, a heterogeneous category of low-status castes (p. 42).

Part 2 deals with the question of the selection of the beneficiary groups. Galanter insightfully illustrates the role of the higher judiciary in developing answers to such difficult questions as what are the appropriate standards and the methods by which the backwardness of these groups is to be measured, and who are the members of those groups. He shows acute awareness of the limits as well as potential impacts of judicial doctrine on group and individual social identities.

In Part 3, Galanter focuses on the "jurisprudence of compensatory discrimination" erected wholly from scratch by the Indian higher judiciary (p. 361). Here the author discusses judicial responses to the questions of how much—how many places can be reserved for how large a class of beneficiaries, how much variation from general standards can be tolerated—and the design of redistributive policies—how to distribute benefits so that all benefits are not monopolized by the better-off, how to build in flexibility, how to articulate preferences for several groups of beneficiaries. In the last chapter of this part, Galanter deals with the actual and potential impact of the judiciary on compensatory discrimination policies. He concludes with observations on the success and costs of compensatory discrimination policies in India and brief comments on the relevance of Indian experiences to the United States.

This book is a major contribution to the study of compensatory discrimination in independent India. It is a masterly and detailed treatment of judicial doctrine and its evolution. Its relevance extends beyond India to all democracies grappling with the problem of ensuring equality to its oppressed minorities while simultaneously guaranteeing equality to all its citizens. Although it is a major treatise on law and society, its major strength—the focus on law—is also its important limitation. Politics is almost entirely omitted. To date, agitation in the state of Gujarat against the reservation policy has claimed more than 300 lives. These and countless other incidents show the deep political and social conflicts engendered by these policies that are beyond the reach of the judiciary. However, these concerns are beyond the scope of this book and do not detract from the fact that it is a definitive study of the law and compensatory policies in India.

UDAY DESAI

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Ethnic Pluralism and Public Policy: Achieving Equality in the United States and Britain. Edited by Nathan Glazer and Ken Young. (Lexington, Mass.: D. C. Heath, 1983. Pp. xi + 314. $27.50.)

Conferences that become books are sometimes an unharmonious pastiche. In this case, the conference seems to have been planned to allow British and American scholars to compare notes on how each nation deals with problems of ethnic equality in law, education, employment, housing, etc. Unfortunately, the essays sometimes talk past, not to, one another. Glazer alludes to this in his introduction when he says that "similar as are the issues raised . . . in the two nations, there are sufficient differences for a simple parallelism of treatment to prove unresponsive to them" (p. 3). No doubt. But here, even the strong parallels that do exist are sometimes neglected, and tangents pursued instead. For example, Bhikhu Parekh's essay assesses several would-be explanations for the underachievement of ethnic minority children in Britain. There is, of course, a parallel American literature. But the essay on race and education in America ignores this literature and focuses instead on such peculiarly American subjects as the recent educational history of Fourteenth Amendment litigation, the proper role for our strong courts to play, and the educational tug-of-war between separated branches of government. Again, there is a good chapter on Britain's failure to end housing discrimination, much less residential segregation. Although the United States has a similar problem—probably for similar reasons—there is no corresponding chapter on the American case.

Exceptions to this pattern of problems unintroduced to one another come from those writers familiar at once with the British and American situations. For example, Christopher McCrudden's chapter on "Anti-Discrimination Goals and the Legal Process" parallels Britain's Race Relations Act of 1976 and Title VII of the Civil Rights Act of 1964. He then draws on such American inventions as class action suits, Brandeis, and *amicus curiae* briefs to show how the impact of the British law might be expanded.

The second notable feature of this book is its stalwart incrementalism. It is not, of course, fair to criticize a book for not doing what it doesn't want to do, but it is important to inform prospective readers of a book's unintentions. Especially in the American half of this book, the question is: Given current structural arrangements and their foreseeable futures, which options make the most sense? Readers looking for a critique of those structures, or for options to, rather than within them, should look elsewhere.

Incrementalism is often born of sanguineness, and this book is full of that, too, again especially on the American side. For example, the final paragraph of Glazer's introduction is an unabashed ode to "the Anglo-Saxon political tradition . . . that is able to accept a remarkable degree of pluralism . . . and which manages to bind together this pluralism with common principles" (p. 6). David Kirp, having reviewed the recently clouded horizon of American education policy, manages to find a silver lining. Although "present prospects are not cheering. . . . The past quarter-century has been marked by almost continuous attention to racial and ethnic minority matters, and a period of respite and consolidation may be in order" (p. 104). And by comparing the American and British race riots of the past two decades to ethnic conflicts in Kuala Lumpur and Northern Nigeria, Donald L. Horowitz is able to pronounce the former "very mild affairs" (p. 206).

In the final chapter, Glazer is quoted as describing "The New World" as a place "where this mixing of peoples into new societies has gone on more vigorously than anywhere else and, I would argue, more successfully" (p. 287). That is a remarkably self-congratulatory reference to a society that "vigorously mixed" blacks into itself via cargo holds and slave auctions, lynched them by the thousands after their "emancipation," segregated them, and continues to consign almost half of all black children to growing up in poverty.

For the most part, the assured and careful scholarship of this book cannot be gainsaid. But the incremental purpose to which that scholarship is put does prompt a demurrer: If not now, when?

CALVIN F. EXOO

St. Lawrence University

Demokratie and Antikommunismus in Berlin nach 1945: Vol. 1, Die politische Kultur der Bevölkerung und der Neubeginn konservativer Politik. By Harold Hurwitz. (Cologne: Verlag Wissenschaft und Politik, 1983. Pp. 395. DM 38.00, paper.)

The first of a four-volume work on democracy and anticommunism in postwar Berlin (two other volumes of which have appeared in the meantime), Harold Hurwitz's study of Berlin's political culture and the new beginning of conservative politics makes an important contribution to empirically based analysis of political behavior in the island-city. He interprets West Berlin's movement toward conservatism as resulting from a sense of relative deprivation that found an acceptable outlet in anticommunism.

The work as a whole uses secondary analysis of public opinion surveys, content analyses of the press, social background studies of elites, and a host of other data to explore Berliners' changing attitudes and behavior from 1945 to 1972. The story is complicated. In brief, however, after the city's conquest in May, 1945 by the Red Army, the bulk of its citizens, like Germans throughout the devastated land, retreated from political involvement at the same time that social democrats and others whom the Nazis had suppressed were seeking to reassert their own right to help create a new, democratic Germany. The eruption of the cold war, with Berlin in its center, saw those Berliners not already under the Soviet thumb more or less eagerly aligning themselves with the Western occupying powers' anticommunism. The demands of this alliance in turn led gradually to an orientation favoring security over innovation, entrenchment over reform. The conservative mood of West Berlin's social democrats eventually came to dominate the national party as well and to encourage the fundamental conservatism of the Federal Republic as a whole.

The first volume emphasizes the two years from 1945 to 1947 (and relies mostly on surveys conducted by the American Military Government's pollsters). Hurwitz first explores the differing basic attitudes of the population at large and the newly rehabilitated opponents of Nazism. The immediate postwar months were grim but not sufficiently so to radicalize Berliners or drive them into apathy. Those who remained in the city were for the most part long-time residents who had a firm grasp on its political realities; wartime as well as postwar changes (such as the loss of Berlin's capital-city functions, administrative and otherwise) had merely strengthened its working-class center of gravity.

Such circumstances, in Hurwitz's view, gave West Berlin, in contrast to West Germany, a good opportunity to realize a social-democratic model of democracy. The fact that the occupying powers—especially, of course, the Soviet Union—restrained Berliners produced only frustration and a sense of relative deprivation. The opportunity to share in the Western powers' anticommunism gave them an appropriate target on which to vent their frustrations. Active participation in the fight against communism also recalled traditional Prussian virtues: self-reliance, responsibility, authority, order, centrality of the family, success as a consequence of struggle and hard work. These exerted an added conservative influence on Berliners. In the process, ideas of democratic reform fell by the wayside.

This volume concludes with a focus on the conservative opponents of Nazism, especially the founders of the Christian Democratic Union, who were quite interested in cementing an anticommunist alliance especially with the United States, and leaves to further volumes Social Democracy, the workers' movement, and the role of the Western allies themselves. These further volumes will also have to test the relevance of Hurwitz's underlying idea of relative deprivation. In the meantime, readers can savor the detailed secondary analysis of data on a wide range of topics, and the fascination of the story Hurwitz has to tell.

RICHARD L. MERRITT

University of Illinois at Urbana-Champaign

Exploring Individual Modernity. By Alex Inkeles. (New York: Columbia University Press, 1983. Pp. xiii + 376. $24.00, cloth; $14.50, paper.)

This book consists of essays on the psychosocial syndrome of characteristics that define modernity. These traits include what Inkeles calls a sense of efficacy, openness to new experience, interest in planning, respect for science and technology, and acceptance of the necessity for the strict scheduling of time. His modernity and other scales were administered to employed males in Argentina, Chile, East Pakistan, India, Israel, and Nigeria. The gist of the findings seems to be that the modern man is essentially the same in all these cultures. He tends to approve of women's rights, to expose himself to the media, to believe in education, to respect the aged, and to be well adjusted. Moreover, the "data indicate that, *in general,* religious and ethnic groups within the same country are not markedly differentiated on a dimension such as modernity" (p. 164), which is a startling finding indeed. Scales of anomie, group hostility, and alienation showed inconsistent and mixed patterns.

Inkeles is apparently trying to make a theoretical as well as an empirical statement. He quickly reviews Durkheim, Marx, Freud, and Weber, noting that they all seemed to think that modernization should lead to terrible things like anomie, alienation, and anxiety. Inkeles clearly thinks they were wrong. The anomie findings are mixed, but Inkeles believes the other findings show that the modern man is less alienated, less anxious, and better adjusted than the allegedly more jealous, exploitative, conflict- and hatred-ridden villager.

There are several reasons why this is not convincing. The unemployed, women, and children are not surveyed. Recent research has questioned whether the Srole Anomia Scale is really a measure of Durkheim's concept of anomie as *dérèglement* (derangement), in that Durkheim accused the status quo of anomie while minorities and the poor seem to be the most anomic on Srole's scale. Freud said guilt, not anxiety, was the price one pays for civilization in *Civilization and its Discontents,* but this insight has never been operationalized. Is it really fair to denigrate primitive and village life as a den of suspiciousness, betrayal, and hatred and elevate modern life so much? What do modern people watch on television—exposure to television is supposed to be a sign of modernity—in the main if not depictions of jealousies, betrayal, violence, and so on? Inkeles ignores the suspiciousness, enmity, and arms race between the United States and the Soviet Union (he regards both nations as modern), but how is this hatred essentially different from hatred between primitive villages? One will find a high degree of these allegedly non-modern traits in the modern social science departments where I have been trained and work, so why pick on peasants?

Inkeles devotes the last two pages of the book to the question of whether modernity is good or evil. He maintains that despite the demonic examples of modern efficacy, planning, and faith in science exemplified by nuclear bombs, strip mining, pesticides, and defoliants, modernity is preferable to traditionalism. He writes:

> It can convincingly be argued that the qualities of the modern man have in their more exaggerated, Faustian, form a potential for producing

> demonic impulses. We would deny that Hitler or Stalin, Khadafy or Amin, in any significant degree embody or symbolize the qualities of individual modernity. (p. 322)

He does not say why these tyrants are not modern, stating only that "acknowledging all this, one may nevertheless believe that without the modern spirit a much poorer future, indeed, one may say a much worse fate, awaits humanity" (p. 322). Why? Certainly no one expects the world to go back to the village as a way of life, but one cannot glibly dismiss the contradictions of being modern, either. The problem with the ideal type of the modern person portrayed here is that he (one wonders how the modern woman would differ) is so shallow and Pollyannish. In the entire book, there is not one bad trait ascribed to the modern man. Open, interested, respectful, accepting, adjusted—these are the adjectives Inkeles uses over and over again. No human, even in the abstract, can be that perfect.

STJEPAN G. MESTROVIC

Lander College

Rhythms in Politics and Economics. Edited by Paul M. Johnson and William R. Thompson. (New York: Praeger Publishers, 1985. Pp. xviii + 342. $38.95.)

Why would anyone be interested in cycles? One answer is that they are repetitious and, as scientists, we are properly disposed toward identifying law-like phenomena. Another is that cycles are, as George Gershwin knew, perhaps the most attractive of metaphors, because they subsume complex phenomena under such a perfectly understandable image. But just as cycles are not causes, these articles raise questions about the relation between rhythm and explanation.

This is the second volume in a series publishing International Studies Association conference papers. The editors have sought coherence by focusing on research aimed at links among recurring economic and political events, but the papers remain quite disparate: some concern political or economic processes but have little to do with cycles; others neglect addressing the question how or why the patterns occur.

The less rhythmic essays are frequently the more interesting. Desmond King and Ted Gurr write on the relative autonomy of the state. Rather than following the literature by studying the policies of national states, however, they view local governments as states and propose that autonomy depends on relations with the national government (in stable delegations of power and the conditions attached to revenue assistance) and with local economic and social forces (in the ability to stimulate and channel economic growth). Although nascent, the approach shows promise for illuminating changes in intergovernmental relations occurring in several advanced nations. Ekkart Zimmerman compares economic trends leading into the Depression and suggests relations among these trends and the ameliorative actions taken by the political authorities in six European nations. Some existing single-country studies better this essay, but the larger project of which it is a part has high potential for unifying measurement and comparison. Barry Hughes tests the hypothesis that the oil shock of the mid-1970s caused increased domestic and international violence. The evidence is generally negative, but with few good measures, only the most impressionistic attempt to control for trends, and a dearth of observations, the verdict is tentative. Mark Amen contrasts Kennedy and Reagan administration orientations toward the interplay between the balance of payments and domestic fiscal policy. Although I am suspicious about its generalizability, this case illustrates the precedence of ideology and intragovernmental bargaining over domestic and international economic conditions as determinants of policy. Robert Kuderle makes a similar claim in explaining American protectionist policies, arguing that adopting a public choice perspective will strengthen the ability of both state-centered and "conventional" approaches to account for the impact of ideology and domestic economic interests on tariff policies. And Mark Rupert and David Rapkin survey the decline of American hegemony, showing with some precision that the two components of this decline—diminishing economic dominance and rising constraints from interdependent relations with other countries—have occurred at quite different rates. Each of these essays is concerned with political and economic phenomena that are in some sense dynamic, but each gains in credibility to the extent that the authors abjure attempts to impose a cyclical perspective.

Other authors take the search for cycles very seriously indeed. Mitchell Kellman and Oded Izraeli seek political business cycles in wage increases in 15 advanced countries over the period from 1964 to 1980. This brief period is unusually dominated by powerful short-term trends (e.g., the OPEC oil embargoes), and the model omits any policy variables, weakening the test at the outset. The results reveal that the election variable is never significant, and R^2 values range from .9 down to a surprising $-.2$. Undaunted, the authors proceed to run various coefficients against each other until they conclude that virtually all the sampled countries are afflicted with such cycles. Robert Elder and Jack Holmes similarly opt for cycles over causes, proposing that American interest in foreign questions and the nation's internationalist or isolationist posture correspond to Kondratieff long waves of economic growth and decline. Plausible but ill-supported, this idea deserves specification and credible measurement. Antoni Moskwa and Timothy Kearney, and Zbigniew Fallenbuchl, track economic cycles in Eastern European nations, concluding that planned investment grows until curtailed by the economics of foreign currency shortages or—more frequently—by the politics of consumer unrest. Charles Doran elaborates the notion of "power cycles" describing the rise and decline of individual countries to international prominence, arguing that the inflection points in these cycles are intrinsically unstable. When several countries pass through such transitional states in a short time period (as before World War I), the result is systemic instability.

The remaining two essays at last hint at the title's potential: They attempt to assemble models that are at once credible yet complex enough in their internal dynamics to explain the initiation and maintenance of oscillations over time. Albert Bergesen proposes a model in which economic expansion, political adventurism, war, and the emergence of a new hegemonic power occur in a Schumpeterian sequence of destruction and renewal. And Pat McGowan focuses on British imperialism in the period from 1870 to 1914 to show how a highly developed laissez faire political economy led to expansionism and thence to international conflict. Rather than the typical hortatory exposition, however, the essay makes some real progress in specifying quantitative indicators and a testable causal model.

Editions like this one are often the home of interesting work in its earliest, most embarrassing state: They demand to be judged on potential. And highly promising ideas, as Gershwin wrote of Bess, are a sometime thing. This volume contains only a few, but they are worth the search.

M. STEPHEN WEATHERFORD

University of California-Santa Barbara

Politics and Policies in Divided Korea: Regimes in Contest. By Young Whan Kihl. (Boulder, Colo.: Westview Press, 1984. Pp. xvi + 307. $32.50, cloth; $11.95, paper.)

Young Whan Kihl's book compares the political development and policy processes of North and South Korea—"regimes in contest" in the divided nation of Korea. The book's eclectic approach, outlined in an introductory methodological chapter, employs typological comparisons, political culture and leadership analysis, political economy, and policy analysis and linkage politics. Kihl succeeds in his goal of mapping patterns of divergence and convergence between the two Koreas and understanding how they came about.

The book is divided into three major sections. The first surveys political developments from the post-World War II division of the country through the consolidation of Kim Jong Il's hereditary succession in the North and the establishment of Chun Doo Hwan's Fifth Republic in the South. The first chapter in this section has little new to offer, but those dealing with "authoritarian adaption" to Sino-American detente in the early 1970s and the "politics of transition" in the 1980s are informative and well done. Among the highlights of the book are Kihl's accounts of Chun Doo Hwan's two-stage coup and factional politics within the South Korean military (including such incidents as the sacking of the Seoul garrison commander in 1981 and the execution the following year of the three-star general commanding the Second Army in Taegu for allegedly plotting a counter coup).

The second part of the book deals with policy patterns and political processes. The chapter on political culture briefly reviews the influence of Confucianism, Yi-Dynasty factionalism, and Japanese colonial totalitarianism on Korean political culture; discusses survey data on contemporary South Korean attitudes towards politics and authority; and speculates on how the "cut-throat, zero-sum, political game of competing legitimacy" (p. 107) between rival political elites has accentuated a tendency towards "supreme leadership." The chapter on political economy is disappointing. Instead of assessing the development models of the two Koreas (which are dealt with in a scattered fashion elsewhere in the book), the chapter serves as a catch-all for such diverse topics as the arms race and the relative resource endowments of the two halves of the divided country. The final chapter in this section provides a good analysis of Chun's consolidation of power and North Korea's opening to the outside world. Kihl's account of how Chun managed to weather a gigantic loan scandal involving his own in-law family in the spring of 1982 makes for gripping reading.

The last section of the book assesses prospects for the future. A chapter on Korean unification reviews the initiatives of both sides since the early 1970s and is as good a short treatment of the subject as one could hope to find. The final chapter outlines a model of political integration and transformation for the two Korean regimes. Kihl concludes that despite different paths of development, the structure and orientation of the two Korean political systems show a remarkable degree of similarity that has been shaped by military competition, their peripheral position in the world economy, a common political culture, and popular demands for stability, modernization, and democracy.

Although the chapters are of uneven quality, Kihl's *Politics and Policy in Divided Korea* is an evenhanded, thorough, and sophisticated comparison of the political systems of North and South Korea. The book will be useful to anyone interested in Korean politics and American policy towards Korea, as well as to those concerned with divided nations and political change in the Third World.

JOHN MERRILL

Lafayette College

The Second Industrial Divide: Possibilities for Prosperity. By Michael J. Piore and Charles F. Sabel. (New York: Basic Books, 1984. Pp. 335. $21.95.)

It has by now become a commonplace among political economists that we are living through a crisis that marks a transition in the history of capitalism. Michael J. Piore and Charles F. Sabel join in the chorus arguing that we are in such a transition period, what they call a "second industrial divide." However, what is novel in their work is their reluctance to engage in deterministic thinking and to predict, as if there were some necessary or unilinear unfolding to socioeconomic development, what the next stage will be like. Instead, they argue that we face a choice about the direction we can move in, and that the choice is in many ways similar to the choice faced by our ancestors at the beginnings of the industrial revolution. The choice is between continuing along a course of development founded on mass-production technology and its related institutions or recharting our course in the direction of craft production. Their preference is clearly for the latter.

The book makes strange reading to one accustomed to the dominant lines of argument in the current discourse in political economy. It breaks radically with both the form and substance of prevailing analyses of the crisis. Piore and Sabel reject completely systemic models of the economy and their attendant laws or logics of development. Rather they opt for a contingent model of historical development wherein technologies, market strategies, and labor control policies are adopted, sometimes accidentally, sometimes due to the exigencies of events, and sometimes as result of political struggles. For example, they claim that the assembly line, the legal framework of the New Deal years, and the rise of mass industrial unions, were a product of some combination of experimentation, expediency, or fairly random events (like the Great Depression or World War II) and not of any logic of capitalism. Much of the support for this view comes from a revisionist reconstruction of U.S. economic history and from a comparison of the experiences of France, Italy, West Germany, and Japan. I found the historical reconstruction too meager and too conjectural to be convincing. However, the comparisons clearly indicate that the same mass production technology can operate within different labor-control systems. Even here, however, Piore and Sabel are forced to concede that there are great similarities between advanced capitalist countries, and one might have expected this acknowledgment to temper their absolute dismissal of deterministic models of development.

In the substance of their arguments the authors relegate the ideas of Adam Smith and Karl Marx to the sidelines. It is the neglected Pierre-Joseph Proudhon who is resurrected and given center stage. Indeed, in their advocacy for craft-based production ("flexible specialization" is the phrase they prefer), one finds a repetition of many of Proudhon's arguments. In the world of flexible specialization they envision, there is no unbridled and cut-throat cost and price competition, for this would stifle innovation and encourage sweat shop conditions. Instead there is a benign form of competition where firms seek to best each other in innovations and yet consent to share among themselves skills and technological knowledge. Nor do Piore and Sabel have much use for entrenched class conflict in this best of all possible worlds, for the struggle between capital and labor produces rigidities in production and divisions within the community. Instead they foresee cooperation and bargaining based on trust, or, as Proudhon put it, based on respect. Thus capitalists would not take advantage of their greater potential mobility to extract unfair concessions from labor because community pressure would remind capitalists of their responsibilities.

Piore and Sabel see evidence that versions of flexible specialization can flourish in the new intensely competitive conditions of the eighties. They point to the dynamism of the textile districts in northern and central Italy, to the success of the mini steel mills in the United States, and to the federated conglomerate enterprises of Japan. All point in the direction of localism and regionalism; of federations of enterprises linked together in communities of respect, with harmony maintained where necessary by the central government. This happy state may or may not follow this period of transition. It all "depends on a thousand imponderables of international politics" (p. 281).

Although there is much to admire in this provocative book (here I think especially of the comparative case studies), the overall argument rests on very fragile supports, and the case made for a "republic of small holders" seeks to dissolve, as if by magic, too many concrete contradictions. One is reminded of Marx's assessment of Proudhon: "he wants to be the synthesis—he is a composite error."

LEON GRUNBERG

University of Puget Sound

Nationalism and Federalism in Yugoslavia, 1963-1983. By Pedro Ramet. (Bloomington: Indiana University Press, 1984. Pp. xviii + 299. $27.50.)

Theories formulated in one sphere of learning can often be modified to illuminate the subject matter of another. In this stimulating volume, Pedro Ramet uses international relations theory to analyze ethnic problems in postwar Yugoslavia.

Ramet argues that "if the federal units of Yugoslavia enjoyed even half as much autonomy as the system's apologists maintained, the pattern of interaction among those units would resemble the pattern of behavior of other collective units operating within a regional system," and "it should be possible to compare the behavioral patterns of Yugoslav federal units and states in the international system" (p. xi). Consequently, the behavior of federal republics and the central government (yet another actor) will be a function of the distribution of power within this system. Ramet derives a set of hypotheses elaborating upon these assumptions with the use of Morton Kaplan's typology of systems of international behavior, specifically the balance of power system.

After descriptions of Yugoslavia's federal institutional structure, the history of Communist party nationalities policy in Yugoslavia, and the policymaking powers enjoyed by the republics, Ramet undertakes a series of case studies of interrepublican and interethnic conflict since 1963. These studies include the economic reform from 1963 to 1971, the emerging eruption of Croatian nationalist and secessionist sympathies from 1967 to 1972, and outbreaks of Bosnian and Albanian nationalism in the 1970s and 1980s.

Not surprisingly, Ramet finds that contrary to Yugoslav claims, nationalism remains alive among the country's minorities. More interesting is analysis that demonstrates that the Yugoslav federal structure, designed to limit nationalist discontent, itself restricts the courses of action that might be taken to ameliorate the sources of ethnic hostility. Finally, Ramet discovers that interrepublican alliances are fluid, which contrasts with the conventional wisdom that believes wealthier republics normally form one bloc and their poorer neighbors another.

Ramet's approach suffers from some of the same pitfalls evident in international relations theory: the units of analysis are not unified actors but comprise a range of policy demands and desires. Indeed, a group of Croatian conservatives opposed the nominal interests of their republic in the crisis from 1967 to 1972. In addition, although accounts of the crisis in Kosovo and descriptions of the economic problems faced by Yugoslavia are interesting in themselves, at times Ramet loses sight of the connection between the details and his analytical framework.

As Ramet points out, controversies yet exist as to whether Yugoslavia is essentially federalist or unitarist. The volume under review weighs heavily in favor of the latter judgment. The book provides a wealth of information on the policy issues faced by the Yugoslavs, interethnic rivalries, and a series of hypotheses that attempt to account for the problems the Yugoslav political system faces in trying to come to grips with the complexities posed by the national divisions in its society. This book should be read by all students of contemporary Yugoslavia, as well as those interested in the phenomena of ethnicity and nationalism.

STEPHEN R. BURANT

The Library of Congress

World Revolutionary Leaders. By Mostafa Rejai and Kay Phillips. (New Brunswick, N.J.: Rutgers University Press, 1983. Pp. x + 237. $29.00.)

This volume describes a sample of 135 revolutionary leaders of 31 violent political revolutions from the English Revolution of the 1640s to several revolutions in progress in 1978, including revolutions from all world regions. It continues, updates, and expands Rejai's 1979 *Leaders of Revolution* (Sage). To gather the necessary data, the authors had to overcome problems of language, official and revolutionary secrecy, and the determined myth-making of both revolutionaries and their opponents.

Despite the importance of the topic of study and the labor involved, the volume has serious practical, theoretical, and methodological flaws, and so contributes little to our knowledge of revolutionaries. Practically, even taking the volume at face value, the reader finds it neither a reference work (e.g., an almanac of revolutionary biography) nor a penetrating analysis of revolutionary leadership. Instead, it is primarily a collection of aggregated characteristics of little inherent interest. For example, what are we to make of the lone facts that 14% of the leaders had only one sibling (p. 53), or that 88% had a strongly positive attitude toward their country (p. 97)? The authors need a control group as contrast, especially for the psychological variables such as "vanity" or "asceticism" that inevitably have loose definitions. The authors acknowledge the control group problem, say they hope to remedy it (p. 16), and make interesting comparisons of pre- and post-World War II revolutionaries. Nevertheless, the lack of a reasonable control group repeatedly undercuts the analysis.

Rejai and Phillips discuss two classes of revolutionary leadership theories: psychological theories (intrapsychic conflicts explain revolutionary leaders' motivation) and situational theories (widespread social turmoil explains revolutionary leaders' emergence). They go on to argue that both psychological and situational factors cause revolutionary leaders to emerge, and note that revolutionary leaders must acquire ideological and organizational skills. The authors then make several ad hoc and unsurprising predictions about revolutionary leaders such as the facts that they will know urban life and be raised in the dominant religion and ethnic group. Because these predictions seem likely to be true of all leaders, and because no control group is considered, the reader can form no conclusions about revolutionaries.

The leaders studied form a convenience sample, not a scientific one. As the authors themselves point out, no good information exists for the revolutionary leaders of Iran, Nicaragua, El Salvador, Thailand, Burma, "and others." Even for the revolutions covered, data were not available for some leaders.

Rejai and Phillips employ a grab-bag of psychological theories; consequently, the analysis appears insensitive, and each theory's individual power is lost. Similarly, the authors pay no attention to the leaders' ideologies beyond a rudimentary classification, despite the obvious importance of ideology to revolutionary politics and leaders.

In contrast to much other work in this genre, the book studies revolutionary leaders for positive as well as negative psychological traits: Leaders can have a "sense of justice-injustice and nationalism/patriotism" (p. 183), not just the irrational feelings of Oedipal rage, relative deprivation, and the like. This admirable impartiality tends to be undercut, however, by the book's unstated, unsupported, normocentric assumption that only revolutionary behavior needs explanation. One might equally ask why, in the face of admitted oppression and injustice, people do *not* revolt? The book's latent message is that revolution is abnormal whereas cooperation is normal. A truly impartial approach demands the consideration of all possibilities.

STEPHEN CHILTON

New Mexico State University

A Woman's Place: Women and Politics in Australia. By Marian Sawer and Marian Simms. (Winchester, Mass.: Allen & Unwin, 1984. Pp. vi-xii + 235. $30.00, cloth; $12.50, paper.)

There is a specific set of circumstances that affect the ways in which ideas are shaped, reshaped, and passed onto members of a social community and feminism, as a political movement, must be understood within the context of factors particular to the vagaries and varieties of a nation's history. At the same time, there seem to be striking similarities between societies, not only those that share a heritage (the British lineage common to the United States, Canada and Australia) but also those that, on the surface, seem distinct from each other. Societies face the same substantive problems of social integration, order, and stability—how to accommodate the demands made by disenfranchised and/or disaffected segments of the population—even if the procedural resolution will be markedly different; and social commentators run interference between abstract conceptions of personhood, epistemologically grounded, and concrete instances of people of a certain race, sex, class, situated at a specific moment in time.

In the broadest sense, then, the study of feminism as a social and political movement is a study of social change—of how and why the social expectations of individuals are called into question, of how and why the purposes and functions of the society are legitimated or devalued. In *A Woman's Place,* Marian Sawer and Marian Simms asks a very specific question: What have women contributed to Australian politics? Their answer, spelled out in terms of both formal politics and submerged or "subterranean" political concerns (that is, moral and/or charitable activities, or issues affecting children and health), is both a specific history of political action by women in Australia and also a recognition of how themes integral to women's movements in other countries colored these activities. For it was not only the case that there was a conscious importation of American debates into the Australian second wave of feminism in the late 1960s but also that issues that colored the American movement were the strands of the Australian tapestry as well: changing patterns of family life; the important recognition that technology was altering or capable of altering the traditional mothering role; employment rights, equitable treatment and affirmative action in the marketplace; issues associated with women's concerns like abortion, child-care, rape and violence, housewifery.

After tracing the struggles for political rights—and most expressly the battles for the suffrage—Australian women got the vote in 1902, although the first woman was not elected to Parliament until 1943—and revealing how these struggles were concerned not only with the simple issue of extending the franchise but also with questions of an ordered sexuality and the appropriateness of women's places and roles as wives and mothers, Sawer and Simms chronicle, in a clear and simple if not very exciting manner, why political representation, expressly the battles for the suffrage, did not follow from franchise victories. The book also details how women have been "hidden from political history," how varied have been the strands of feminism, and how distinctive have been the political parties' reactions to the women's movement depending on a confluence of organizational, personal, and historical factors.

As is the case in other feminist discourse, the question is raised as to what consequences follow from female representation in politics, that is, whether extending the political universe to previously disenfranchised groups merely opens up doors to those for whom they were once closed and equalizes the political arena, or whether a new dimension to the political is introduced. In the case at point, it is whether women *qua* women bring an element of "care" or "nurturance" to politics; that is, whether being a woman, either through anatomical factors or the social meaning imparted to those factors not only assures a bonding with other women—a female cohesiveness if you will—but also that it affects how one assesses issues of war, peace, ecology, and technology. Nothing in the Sawer and Simms books validates their hypothesis that when enough women become involved in politics, when a "critical mass" is reached, the ethic of care will revolutionize the political game. The portraits of women members of Parliament, which make up a good part of this book, as well as the data on changing characteristics of women politicians, the portfolios that they hold, and their links to certain networks, offer no conclusive proof that there is an inextricable tie between women, morality, and the ethic of care, will it as some feminists might.

SONDRA FARGANIS

Vassar College

Revolution and Counterrevolution in Central America and the Caribbean. Edited by Donald E. Schultz and Douglas H. Graham. (Boulder, Colo.: Westview Press, 1984. Pp. xv + 555. $35.00, cloth; $14.95, paper.)

Current struggles in Central America and the Caribbean are complex and have multiple causes. As such, they can be understood only through complex and multidisciplinary approaches. This is the major theme of Schultz and Graham's edited volume on Central America. As a theme it may not be new, but the authors have managed to edit a work that reinforces this theme and deepens our understanding of the recent crises in Central America.

The book is divided into three major sections: one dealing with structural and institutional interpretations of Central America, one focusing on individual countries in the region, and the third dealing with the international dimensions of the struggles in Central America. The first section features an excellent introductory chapter by Donald Schultz, "Ten Theories in Search of Central American Reality." This chapter alone makes the book worthwhile, as Schultz analyzes clearly our attempts (from relative deprivation to the "domino" theory) to make sense of the Central American reality. He provides a convincing argument that we are too willing to force fit Central American realities to our models. In this process, reality loses.

This section also includes an essay by Thomas Karnes (chap. 2) on the history of U.S. involvement in the Caribbean, a study by Douglas Graham (chap. 5) on the factors causing economic decline in Central America, an essay by Paul Heath Hoeffel (chap. 3) on the oligarchies of Guatemala and El Salvador, and a chapter by Penny Lernoux on the Church in Central America (chap. 4). Karnes provides a brief (perhaps too brief) history of the U.S. role in the region. Graham's analysis focuses primarily on the economic causes of instability (as his title claims), where he places blame on excessive government involvement in the economy, overvalued currencies, and import substitution industrialization. Hoeffel studies Central American oligarchies and argues that the current struggles in the region spring from the oligarchies' reluctance to accept their inevitable downfall. Lernoux has written a poignant narration of the struggles of the Church in Nicaragua, El Salvador, and Guatemala. Reading her essay makes one unable to accept the simplistic and stereotypical explanations of Central American realities.

The second section of the book presents in-depth studies of individual countries of the region. By far the best of these is Donald

Schultz's essay (almost a monograph) on El Salvador (chap. 6). It provides a clear and nearly comprehensive treatment of the country. Its only shortcoming is the author's insistence on placing odds on the likely futures for El Salvador and the region. In another chapter Mark Rosenberg (chap. 9) provides a brief analysis of civil-military relations and of the refugee problem in Honduras. Gordon Bowen (chap. 7) studies the frequently ignored development of state terrorism in Guatemala. In chapter 8, John Booth provides a sympathetic explanation of the Sandinista revolution in Nicaragua. Claudio Gonzalez-Vega (chap. 10) studies Costa Rica's economic policies of the 1970s and claims that they have created the potential for social unrest and violence in the near future. Carl Stone (chap. 11) looks at the development of political party ideology and economic policy in Jamaica in the 1970s.

The final section includes four chapters on the international dimensions of Central American crises. Dennis Hanratty (chap. 12) looks at Mexican foreign policy, Robert Leiken (chap. 13) studies the Soviet and Cuban roles, Wayne Smith (chap. 14) analyzes what he sees as the disaster of Reagan's foreign policy in Central America, and Donald Schulz (chap. 15) offers some policy prescriptions for the United States in the region. Seen in the context of the complex issues discussed earlier in the book, the chapters in this section seem quite simplistic and superficial. Overall, Schulz and Graham have put together a valuable and understandable tool for making sense of Central America. They are to be congratulated.

BRIAN F. WALLACE

Capital University

Ethiopia: Politics, Economics and Society. By Peter Schwab. (Boulder, Colo.: Lynne Rienner Publishers, 1985. Pp. xx + 134. $18.50, cloth; $10.00, paper.)

This book is the first in a series of 36 multidisciplinary volumes on Marxist regimes promised by Lynne Rienner Publishers to be published in the next few years. The purpose of the series, according to the publisher, is to examine and evaluate "critically the application of Marxist doctrine to the respective societies, assessing its interpretations, its success and failures." As one examines Peter Schwab's contribution to the series, the inevitable conclusion is that the Marxist regime in Ethiopia has been largely successful in implementing its policies. This conclusion is derived by looking at the issues that Schwab discusses in the four substantive chapters: "History, Political Tradition and Social Structure"; "The Political System"; "The Economy"; "The Regime's Policies"; and the conclusion.

In terms of history, political tradition, and social structure, Schwab argues that in the years following the 1974 overthrow of Emperor Haile Selassie, the three centers of power in traditional Ethiopia—the Ethiopian Orthodox Church, individual landlords wielding tremendous autonomy, and the Islamic church in the north of the country—have all been effectively neutralized by the regime that took over the reigns of government. The political success of the regime, according to Schwab, can be seen, for example, through the abolition of the Orthodox Church as the official state religion, a status it had enjoyed under Haile Selassie and his predecessors.

Schwab sees the model created by the Dergue (the Coordinating Committee of the Armed Forces, Police, and the Territorial Army) in Ethiopia as having the potential to influence other African countries in terms of implementing a revolutionary strategy. In this model, the armed forces usurp power from what Schwab would consider to be a reactionary leadership (or government) and quickly move to implement fundamental and all-encompassing changes in society the way it was done in Ethiopia. Schwab gives the example of Liberia under Master Sergeant Samuel L. Doe (p. 111) as potentially moving in that direction. However, the events of the last two years in Liberia, where Doe and his allies have become increasingly repressive and erratic not to mention the noninstitutionalization of fundamental reforms in society, should caution against theorizing on the duplication (or replication) of the Ethiopian model elsewhere.

Schwab's discussion of the Ethiopian economy is less generous than his discussion of the newly created political system. Despite a political shift in class power in Ethiopia from the landlords and the church to the working class and peasants (represented by the state), the economy has been in a sorry state since the revolution. Despite decline in production, agriculture still predominates, accounting for almost all exports. The decline in agricultural production is attributed to two interrelated sets of factors, one internal and the other external. Internally, the political turmoil or disruption resulting from land redistribution to the peasants and resistance by previous landowners accounts for the sharp decline in production of both subsistence and marketed crops. External factors include industrial inflation, high oil prices, and inelastic international market factors that are largely beyond Ethiopian control. The latter factors should have a sobering effect on those who perceive political victory by the oppressed classes as being the beginning of economic prosperity. In short, on the morrow of a political revolution, an underdeveloped country still remains one. Schwab correctly observes that the current drought in Ethiopia should not be attributed to the Marxist regime, because even under Haile Selassie Ethiopia suffered a serious and crippling drought.

It would be an understatement to say that Schwab looks at the Ethiopian regimes policies in essentially a very positive light, including the very controversial (and emotional) policies relating to the nationalities question in Eritrea, Ogaden, and to a lesser extent, Oromo. As the author points out, the incorporation of Eritrea and Ogaden into the Ethiopian empire had been achieved through Haile Selassie's skillful manipulation of post-World War II international politics. It is clear that the current regime has adopted a policy of removing "national" barriers, instituting a policy of total incorporation of both Eritrea and Ogaden in the larger Ethiopian political framework. Schwab points out that opposition to the regime's policies has been significantly defeated militarily. This has minimized the chances of any successful campaign for autonomy. A shortcome in Schwab's analysis of this question is that he does not in a serious way discuss the merits of the Eritrean question in terms of rights of nationalities for autonomy and self-determination.

Finally, the issue of violence. Ethiopia has been condemned in the international arena for the level of violence that the state has been willing to use against its opponents. Schwab, however, argues that the level of violence was necessary (if not inevitable) given the intense class contradictions existing in the *ancien regime.* That may be so, but Schwab underestimates the potential for compromise that existed in the earlier years of the revolution given the (sometimes expedient) support the new regime enjoyed from most political groups in the country, including Eritreans. After 1978, it is easy to see why the regime was not willing to compromise, given its calculation of the inevitable defeat of the opposition. In any case, one would have to agree with Schwab on the success of the regime in attaining its objectives: land redistribution, creation of peasant and urban associations, and finally in September, 1984, the creation of a worker's party. The cost in human lives, however, has been enormous.

JULIUS E. NYANG'ORO

The University of North Carolina at Chapel Hill

Three Visions of Chinese Socialism. Edited by Dorothy J. Solinger. (Boulder, Colo.: Westview Press, 1984. Pp. xii + 143. $16.00, paper.)

Although the authors would undoubtedly disagree, one can see this volume of essays as an attempt to replace one cliche with another. The five China specialists who contributed to this collection discovered, somewhat accidentally, that they were all questioning the value of the "two-line struggle" model for understanding Chinese politics and, in addition, were all coming to believe that Chinese politics could best be seen in terms of a "three-line struggle." Their views were originally presented at the 1982 meeting of the Association for Asian Studies and later updated for this publication.

As Carl Riskin notes in his introduction, "the 'two-line' struggle must surely have been one of the most frequently published cliches in China during the 1960s and early 1970s" (p. 1). During this period the Chinese insisted that their politics were dominated by a struggle between radicals adhering to the Maoist line and those following the moderate line associated with Liu Shaoqi (which was scornfully labelled the "capitalist road" by the Maoists). In contrast the authors argue that there was, and continues to be, a struggle among adherents of three lines: the Maoist line "with its social mobilization and mass movement orientation, its aversion to professional bureaucracies and its attempt to decentralize administrative power"; the Soviet or bureaucratic line that advocates central administrative planning and control; and the "market socialism" line that rejects the command economy (p. 1). Edward Friedman argues in "Three Leninist Paths within a Socialist Conundrum" that none of the models have succeeded in realizing true socialism because of the decisive power of the Leninist party. Friedman maintains that the "dominant reality guarantees the only limited success of campaigns against bureaucratism and the other ills of the Leninist Party-states. . . . The system tends to be self-limiting" (p. 36). Rather than seeing Maoism as a cure for Stalinism, Friedman sees the two converging into "malignantly murderous forms of Leninist-Party states" (p. 19). Although Friedman may be correct, it seems inappropriate to equate the policies advocated by Liu and other moderates with Stalinism. If Mao himself became China's Stalin, then where does the Liu line fit in Friedman's categories of Stalinism, Maoism, and Titoism?

In his turn Richard Kraus, writing on "Culture: Cultural Politics and the Political Construction of Audiences in China," interprets the conflict over the arts as a power struggle in which "the distribution of cultural resources is treated by political leaders as a process of enhancing the strength of the constituencies favored by their own vision of what kind of society China is and should become" (p. 65). Thus the radical Maoists want to distribute culture among the masses, while the bureaucrats want culture allocated by the state and the Party.

Writing on "Three Lines in Chinese Foreign Relations, 1950-1983: The Development Imperative," Peter Van Ness argues that the domestic line in ascendance at a particular time has determined China's international economic policy and, hence, critically influenced its overall foreign policy. In the most detailed and solid essay, Dorothy Solinger focuses on "Commerce: The Petty Private Sector and the Three Lines in the Early 1980s." Her points seems well taken that the frequent variations in the treatment of small-scale commercial activity and the conflict over the appropriate treatment can be usefully interpreted in terms of marketeer, bureaucratic, and radical attitudes.

Although as its proponents concede, it is difficult to find much evidence of the market line in the period before Mao's death, the three-line model has to be considered an improvement over the two-line version. However, the three-line model is itself too simple and forced. Scholars and sophisticated journalists have not universally accepted the Chinese two-line view. Instead, a variety of interpretative frameworks have been employed ranging from that of bureaucratic politics to factional models and policy arena analyses. Chinese politics are too complex, and our knowledge has too many gaps for any one of these interpretative frameworks to be wholly satisfactory. To force Chinese politics into the mold of a three-line struggle seems to offer little more than a new cliche as a substitute for the discredited one.

ANNE FREEDMAN

Roosevelt University

Political Society in Developing Countries. By A. H. Somjee. (New York: St. Martin's Press, 1984. Pp. xii + 202. $27.50.)

A. H. Somjee's most recent book sets out for itself the ambitious task of explaining the advancement of liberal political values and institutions in developing nations. Eschewing more conventional explanations in development theory, Somjee adopts an eclectic approach emphasizing the vital role of national elites as educators and transmitters of attitudes and behavior appropriate to sustaining liberal-democratic values. As in his previous work, India is the basic point of reference. The book proceeds in four major chapters, dealing respectively with the growth of political society in the West, the growth of political society in India, normative-pragmatic consideratons in political involvement, and problems of political society in comparative perspective.

His thesis, as I understand it, is quite conventional. Political society—defined as the social arena lying between ascriptive groups and legal-political institutions, to include parties, interest groups, and other political elites—must adopt a style of political behavior balancing normative commitments with pragmatic assessments of what is politically possible given a commitment to liberal-democratic institutions. Somjee's notion of the normative-pragmatic preconditions of liberal-democratic society borrows from Gabriel Almond and Sidney Verba's *The Civil Culture* (Princeton University Press, 1963) and other well-known examinations of democratic political culture.

Although the effort to develop useful generalizations about the preconditions and strategies for promoting liberal-democratic institutions in developing nations is commendable, Somjee's book suffers a number of serious flaws. The indicators of democratic political society are nowhere defined with precision or examined empirically. Although he alludes to the concept of "political culture," Somjee otherwise fails to utilize the vast comparative literature about this theme in developing his thesis.

Somjee's attempt to comparatively analyze the problems of creating and sustaining liberal political societies is similarly disappointing. He abdicates the chore at the outset, announcing that "it is impossible to formulate a common framework which can help us to compare the problems of building a political society conducive to the establishment and maintenance of responsible and accountable government in 115 diverse developing countries" (p. 125). His highly descriptive and ad hoc discussion is punctuated regularly with normative injunctions on what ought to be, which detracts from his efforts at explanation. And his treatment of selected countries in East Asia, the Middle East, Africa, and Latin America is cursory and based on a selective reading of the relevant area studies.

What strengths the book has are found in Somjee's examination of the role of India's political elites—Mahatma Gandhi, Jawaharlal Nehru, Sadar Patel, Jayaprakash Narayan, and others—in promoting certain ethical ideals and pragmatic approaches to the political problems India encountered in the pre- and post-independence eras. Somjee's treatment here is largely descriptive but will prove useful to generalists interested in the intellectual foundations of the modern Indian state.

It is not at all clear, however, how India's experience is relevant to understanding the obstacles and possibilities for advancing liberal-democratic institutions in, say, Nicaragua or Nigeria. Few would take exception to Somjee's point that effective leadership is vital in promoting Western liberal values in non-Western, non-liberal societies. But Somjee begs important questions. What conditions are more or less conducive to effective democratic leadership? What political strategies serve best to promote liberal values in designated circumstances? What institutional arrangements are best suited to certain types of sociopolitical conditions? We are left in doubt on these and many other questions.

In sum, this work fails to match the promise of its title. It might better have been cast as a case study of the influence of India's leadership on domestic political culture rather than asserting theoretical and comparative pretensions.

STEPHEN P. MUMME

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Bureaucrats and Policy Making: A Comparative Analysis. Edited by Ezra N. Suleiman. (New York: Holmes & Meier Publishing, 1984. Pp. 279. $37.50.)

Bureaucrats and Policy Making contains eight papers presented at a December, 1980 conference in Madrid, which examine the relationship between higher civil servants and the government in power in the United States, Italy, Japan, France, Britain, Germany, Norway, and Chile. The book sets out to study the degree of influence that civil servants exert on the political process in each of the nations. The volume describes the recruitment practices, structure, and the political role of the bureaucracy in each of the nations.

The volume is valuable for those with little prior knowledge of how the higher civil service operates in different countries. Because the career civil service in Britain, France, and Japan is frequently pointed to as an interesting contrast to the United States' higher career civil service system, the book provides an increased understanding of each of those systems.

The chapter on the United States is written by Hugh Heclo. Although it covers some of the same ground discussed in Heclo's well-known book, *A Government of Strangers* (Brookings Institution, 1977), the chapter presents an excellent overview of the American higher civil service. Heclo concludes that a major characteristic of the American system is that it is not a system at all. In contrast to systems such as the British civil service, there is little mobility among United States senior executives and a lack of government-wide orientation. A major characteristic of the British civil service system, writes Richard Rose, is that there exists a "Whitehall community" that identifies strongly with government-wide service and **not** with individual departments. The United States, according to Heclo, has not yet discovered the means of meshing its higher civi service with presidential leadership and developing a longer, broader, and government-wide perspective.

If the book has a weakness, it is that there is no concluding chapter. Although Suleiman does provide an introduction, the chapter does not attempt to make any conclusions or comparisons between the eight nations examined. One who approaches the book looking for lessons or good practices will be disappointed. Instead, the burden is placed on the reader to develop his or her own comparisons between the systems. The concluding chapter might have attempted to answer Suleiman's opening question about the influence of career civil servants on the politics of each nation. Could a continuum have been developed that places each of the eight nations along a spectrum ranging from high to low influence? Where are civil servants strong and where are they weak, and what are the determinants of their relative strength?

Although a concluding chapter would have greatly strengthened the book and made it more than just a compilation of papers, the volume is still highly worthwhile and very informative. As Suleiman points out in his introduction, the role of career civil servants in the policy process has been receiving considerable attention in recent years. Such attention is highly appropriate and overdue. As the eight chapters clearly illustrate, higher civil service systems are now an essential fact of life characterized by an ambiguous relationship between the higher civil service and the government in power in most countries. The ambiguity and tension betwen bureaucrat and politician, although clearly an inherent part of government, can either be functional or dysfunctional. In the chapter on Japan, T. J. Pempel argues that that country has developed an effective balance between the extremes of total bureaucratic control and total political control. In the case of the United States, Hugh Heclo argues that the balance has not yet been found. This volume can make an important contribution in the United States if it further stimulates the debate on how the higher civil service system in the United States can find that effective balance.

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Haiti: Political Failures, Cultural Successes. By Brian Weinstein and Aaron Segal. (New York: Praeger Publishers, 1984. Pp. xi + 167. $25.95.)

Haiti is the starkest reminder that contrary to current ideological tirades, merely "authoritarian" regimes also generate boat people. The "land of unlimited impossibilities" also constitutes a fascinating case in which capitalism, working under textbook conditions (practically no direct taxes, no minimum wage to speak of, no unions, no government regulation of the economy) has turned what was in the eighteenth century one of France's richest colonies into what is today the poorest country in the Western Hemisphere and one of the poorest in the world, with a per capita income of $200 and an infant mortality rate of 140 per thousand.

Although a number of English-language historical surveys of Haiti were published in the seventies, the need for a recent study of current Haitian politics has been evident for some time, and this volume in the Hoover Institution series on Latin American politics is an effort to fill that gap. Somewhat in tradition of the country studies commissioned by the U.S. State Department, it provides a useful summary of Haitian historical developments plus chapters on institutions ("government by franchise"), the economy ("coffee and baseballs"), Haiti's insertion into the international system, as well as some final reflections on the key challenges facing the world's first black republic and on the unique character of this fascinating society that stubbornly refuses to fall into any neatly arranged ideological pigeonholes.

Not surprisingly, the most interesting sections are those dealing with the workings of the Haitian political system, taking us beyond the facile reliance on repression as an all-explaining variable to account for the remarkable ability of the Duvaliers to remain in power for almost three decades. According to Weinstein and Segal, "government by franchise involves the assignment of government functions to trusted persons and families within the central government in cities and throughout the rural areas" (p.50). Thus "personalization is the key to power and resources" (p. 51). The emergence of a network of middle-men, primarily in the countryside but also in the cities, dependent on the government's goodwill to be able to continue their functions as economic intermediaries was crucial for Francois Duvalier's consolidation of power and his ability to stand up even to the Kennedy administration's efforts to oust him. "As a result, factions have been far more important than political institutions such as parties or trade unions. Universalistic ideologies or belief systems, long-term programs for political or economic development, and even educational resources are irrelevant. This is pirate politics on a rudderless ship" (p. 103).

But it is perhaps precisely because of these and other useful insights into the workings of Haitian politics that the book as a whole is somewhat disappointing. Rather than elaborating and fully developing their argument around these insights, Weinstein and Segal quickly move on to their description of other aspects of Haitian society, seemingly intent on compressing as much information as possible into 150 pages of text. Chapter subheadings seem more like laundry lists ("tourism," "infrastructure," "external trade") rather than parts of an overall theoretical or historical argument. Although informative and well-documented, the book thus fails to make a real breakthrough in furthering our understanding of Haitian politics, an objective that is also made difficult by the authors' almost exclusive reliance on secondary sources.

JORGE HEINE

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International Relations

Military Development in Africa: The Political and Economic Risks of Arms Transfers. By Bruce E. Arlinghaus. (Boulder, Colo.: Westview Press, 1984. Pp. xiv + 152. $23.50.)

This brief but comprehensive treatment of political and economic considerations of arms transfers to Africa appears to have benefitted from computer searches of reference material. In 99 pages of text Arlinghaus uses 413 citations, sometimes several per sentence, and little repetition of sources. The result is a condensation of a mass of material written about African military development, in some cases putting the African situation in a broader context of developments elsewhere in the Third World.

There is no evaluation of whether military expenditures are normatively appropriate. Rather it is assumed that money will be spent on defense, and the focus is on motivations, reasons for success or failure, and appropriateness of weapons systems for the desired end of national security. Remarks in the Introduction concerning five myths of military development (the military as political modernizers, the military as nation builders, that militarism is rampant in Africa, that disarmament will lead to economic development, and that new technology in conventional weapons will reduce costs) imply a rejection of normative arguments on priorities from both the political Left and Right. But the assertion that "this linkage between military and economic development should be recognized and accepted as such" (p. 4) also implies an acceptance of the status quo in terms of recent trends in military spending.

Arlinghaus can avoid taking sides on such issues because, although general conclusions are reached on page after page, they are not specific enough to indicate when sufficiency is reached in terms of security, or training, or technology. Nevertheless, the book is an excellent analysis of the costs, benefits, and risks of continued arms transfers to Africa. These considerations are analyzed from the point of view of both supplier and recipient. He concludes that the "pull factors" creating demand for weapons are more important than the "push factors" and interests of the supplying nations. Perceptions of domestic and external threats coupled with a need to demonstrate independence as a sovereign nation appear to be the major determinants of demand. The costs and risks are high, however, because of unfavorable terms of trade, risks of high indebtedness, need for foreign technicians, and inappropriate matchups of weapons with existing arsenals and manpower availability and capability.

These costs and risks are higher with so-called "new conventional weapons" involving the latest technology. Such weapons are attractive because of their deterrent value against external threats. Their actual value may be negative, however, because the problems noted above would be aggravated, and actual security capability reduced in the process. A number of case studies have shown that weapons that could not be used effectively because they could not be transported on existing rail lines or roads, or could not be used or maintained because of inadequate infrastructure (of education, managerial skills, or economic base) that may have had symbolic value when a sales agreement was announced, but actual delivery created a series of unintended and unforseen problems.

The answer might be to develop appropriate military technology suited to early stages of industrialization. But although there would be a better fit with existing resources, the problems may be insurmountable. In foreign markets there is little demand for intermediate technology items, hence little supply or support or training or credit readily available. If produced domestically it would compete for scarce resources and skills in the civilian economy.

The options for African nations trying to provide both military security and econmic growth are few and fraught with difficulty. The options for U.S. policy are also few and unattractive. Attempts to determine "applicability" of arms and impact of arms transfers have not met with much success. And suggestions to increase U.S. military aid for weapons, infrastructure, and human capital development will create alarm in Congress as the budget increases, as well as among those concerned about the current arms trade. This book is useful in showing where African military development is heading and where the pitfalls lie. Policymakers may not learn the answers to their problems, but they will certainly better know the risks involved in various policy options.

ROLAND F. MOY

Appalachian State University

The United Nations: Reality and Ideal. By Peter R. Baehr and Leon Gordenker. (New York: Praeger Publishers, 1984. Pp. viii + 214. $28.95.)

As the United Nations enters its fifth decade, it is useful to take stock of its progress or lack thereof. Accordingly, Baehr and Gordenker offer a pithy review of the past 40 years. Their essay, however, presents little that has not been said already and in greater detail by such distinguished students of the world organization as Inis Claude, Jr., *Swords into Plowshares* (1971), H. G. Nicholas, *The United Nations as a Political Institution* (1975), and A. Leroy Bennett, *International Organizations* (1984), among numerous others.

Baehr and Gordenker divide their analysis into eight short chapters: "The Origins of the United Nations," "Structure of the United Nations," "Membership and Decision Making," "Maintaining International Peace and Security," "Human Rights," "Non-Self-Governing Territories," "International Cooperation for Development," and the conclusion, "Ideals and Realities." A copy of the Charter of the United Nations, a modest bibliography, and an index follow.

There are no footnotes in the entire text. One also wonders why the authors did not mention even a single item from the important and extensive periodical literature in the bibliography. Finally, the entire volume would have benefited from one more proofreading.

On the positive side, the analysis flows smoothly and reads well. Baehr and Gordenker, for the most part, are also accurate with their facts and state their case incisively. The two authors blame the sovereign state system, not the world organization, for the present situation in which "the United Nations has not achieved what many statesmen and ordinary people all over the world sought in 1945" (p. 163). They argue well in their conclusion that "Sanctions fail not, because of the lack of an organizational apparatus, but for want of commitment by principal members to carry out the necessary programs and make the necessary sacrifices" (p. 173). In another context, they similarly declare: "If the use of military means in politics can be considered a threat or breach of the peace, as the UN Charter suggests, both the superpowers repeatedly threatened the system" (p. 97).

Although admitting that, in regard to human rights, "it is not completely without reason that some observers have accused the United Nations of applying a double standard" (p. 116), Baehr and Gordenker maintain that "the United Nations can claim the positive accomplishment of making the norms [of human rights] sufficiently concrete so that it is possible to determine where and when they are violated" (pp. 114-115). Analogously, the authors assert that the UN's often criticized goals for development constitute "norms [that] do set an ideal standard against which performance can be measured" (p. 156). Interesting too is their comment that at the San Francisco Conference "the name United Nations was officially adopted, despite misgivings by France whose delegates pointed out that the initials 'NU' meant naked in their language" (p. 16).

For the most part, however, the book is largely repetitious of earlier analyses, and its findings are rather mundane. In addition, although there can be no doubt that Dag Hammarskjold did play a noteable role, a fact which Baehr and Gordenker emphasize on several occasions, they fail to mention that Hammarskjold also extended his activities so far that he almost ruined the office of the Secretary-General as well as the organization itself. What is more, although the problem of Rhodesia often seemed intractable and "the history of the issue [of Namibia] since 1946 shows that a powerful and stubborn opponent can frustrate the will of the governments of the world" (p. 131)—it is simply not true that both "in the cases of Namibia and Rhodesia . . . the United Nations could not contrive to gain its goals" (p. 136). The final emergence of Zimbabwe owes something at least to the world organization.

Finally, one should note that South Africa has been excluded from the General Assembly on the grounds that its credentials are invalid since 1974, not "since 1979" (p. 111); the United Nations has six *official* languages, not "six working languages" (p. 59)—there are two *working* languages, English and French—and Ho Chi Minh died in 1969, so that his government could not have been operating "in 1975" (p. 45).

MICHAEL M. GUNTER

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To Promote Peace: U.S. Foreign Policy in the Mid-1980s. Edited by Dennis L. Bark. (Stanford, Calif.: Hoover Institution Press, 1984. Pp. xxviii + 298. $19.95.)

To Promote Peace is a collection of 17 essays written in 1983 under the auspices of the Hoover Institution. The book's purpose is to "generate debate and disagreement" on U.S. foreign policy in the 1980s, and it examines America's economic, diplomatic, and military-strategic ties to Western Europe, its regional interests in the Third World and the Pacific Basin, and the relations between the two superpowers.

The economic challenges facing national authorities in open economies are considered in essays by Ronald McKinnon and Gerald Dorfman. McKinnon provides an excellent overview of recent world monetary developments and proposes that the world money supply be jointly managed by the United States, West Germany, and Japan. But the problems besetting this proposal—the residual role of the dollar in the system, the need to reach and sustain a consensus on the world inflation rate, and the administrative setting of "equilibrium" exchange rates—are not satisfactorily resolved. In his discussion of economic policymaking, Dorfman demonstrates that high unemployment has broken union militancy and removed a contentious interest group from the economic policy process in Western Europe. And he squares the circle by reasoning that high unemployment will not only ensure coherent and effective economic policies, but will also promote political stability.

The growing incongruity between the strategic interests of the United States and its European allies is explained away by Melvyn Krauss with the concept of defense free-riding, which he believes has encouraged European pacifism, subsidized European trade with the Soviets, and is undermining NATO. Krauss proposes to reverse these trends with the unlikely promotion of West Germany to the status of a "full nuclear partner with the US in confronting the Soviet menace" (p. 127). H. Joachim Maitre embraces the free-rider argument as well, but offers the more conventional recommendation of increased European defense spending and the broadening of European responsibilities outside of the North Atlantic area.

The Middle East and the putative threat posed by the Soviet Union to Western oil supplies are considered by Edward Teller and George Lenczowski in separate essays. Edward Teller advocates energy autarky, whereas Lenczowski favors American political support of the southern Persian Gulf states to guarantee an uninterrupted supply of oil to the West. Robert Wesson's thoughtful essay on Latin America concentrates on American interests rather than Soviet expansionism. He links American security to the economic development of Latin America and more significantly to the "reincorporation of Cuba and its allies in the hemispheric order" (p. 140). In other essays examining America's regional interests, Ramon Meyers proposes that the rearmament of Japan, in exchange for an American commitment to free trade, would best serve America's strategic interests in the Pacific Basin; and Peter Dunigan depicts Soviet policy in Africa as a calculated effort to gain control over strategic "choke points" which, if realized, would threaten the economic security of the West and the cohesion of NATO. And finally, Melvyn Krauss's essay on foreign aid attributes the grinding poverty of many third-world countries to government support of infant industries, the Western welfare state, and even foreign aid itself.

In this collection of essays, the Nixon-Kissinger perception of the Soviet Union as a great power driven by the dictates of the realpolitik is generally rejected, and the sources and objectives of Soviet foreign policy are derived from Marxist-Leninist ideology. Robert Conquest details this particular view of Soviet foreign policy and recommends that the United States return to what is the fundamentally flawed policy of the global and undifferentiated containment of the Soviet Union. Milorad Drachkovitch's contribution retrogresses further in a proposal to roll back the iron curtain in Eastern Europe.

In his essay on American strategic and arms control policy, William R. Van Cleave claims that President Carter opened a window of vulnerability that has not yet been closed by the Reagan administration. He criticizes the decision to deploy the MX missile, favors the development and deployment of the Midgetman missile as well as increased spending on strategic nuclear forces, and is critical, if not hostile, to arms control.

Arnold Beichman's indictment of the American intelligence community concludes *To Promote Peace.* He refers derisively to Congressional oversight of the CIA as "niggling hindrance" and leaves the impression that the CIA should model itself on the KGB in its intelligence gathering activities.

Dennis L. Bark could have exercised his editorial powers to the advantage of both reader and author alike: Several essays in this collection are needlessly abrasive; some authors are viscerally intolerant of views contrary to their own, particularly the contribution by Alain Besançon and L. H. Gann on pacifism and the peace movements in Europe; and James Bond Stockwell's largely autobiographical essay on leadership doesn't really leave much room for debate, only disagreement—a fault shared by many of his fellow authors. This collection would have been more engaging had the authors encouraged debate. But debate is not possible when assumptions, and in the end conclusions, are treated as self-evident truths.

JAMES CLYDE SPERLING

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National Security and International Stability. Edited by Bernard Brodie et al. (Cambridge, Mass.: Oelgeschlager, Gunn & Hain, Publishers, 1983. Pp. vi + 441. $30.00.)

The contributors to this volume, according to the introductory chapter by editors Kolkowicz and Intriligator, "were asked to consider the formative ideas, theories, and policy prescriptions of the 'golden age' of strategic studies, to reflect upon these ideas and theories from a two—or three—decade long perspective, and then to provide their evaluations and reassessments" (p. 3). The editors go on to say that "the contributions to this volume reflect the high intellectual standards espoused by Bernard Brodie . . ." (who died in 1978). One wishes that this were so. Instead, this volume is an evidently much delayed collection of essays of varying quality without much connection to each other or the editors' stated purposes.

If the book is viewed at the more ordinary level of loosely organized collections of national security essays, the quality of some of the individual contributions is still disturbingly low. Even such well-known authors as Edward Luttwak, "The Operational Level of War," and Robert Jervis, "Deterrence and Perception," have made disappointing contributions—opaquely written (Luttwak) and seemingly thrown together without any evident conclusion (Jervis). The essay by Thomas Schelling, "The Terrorist Use of Nuclear

Weapons," seems to be an unorganized collation of random thoughts. The essay by Henry Trofimenko (of the Soviet Academy of Sciences), "Changing Attitudes towards Deterrence," is a litany of complaints about the United States, perhaps included as a token Soviet perspective. Alternatively, Peter Paret's essay, "Revolutions in Warfare: An Earlier Generation of Interpreters," concerns itself with early nineteenth-century military strategy. Although interesting and well written, one has to question what an essay on that topic has to do with "the golden age of strategic studies."

The volume is partially redeemed, however, by a few interesting essays. Desmond Ball's useful "The Role of Strategic Concepts and Doctrine in U.S. Strategic Nuclear Force Development" contains little that is new but constitutes a coherently written brief overview of U.S. strategic doctrine since World War II. Alexander George's "Toward a Soviet-Ameican Crisis Prevention Regime: History and Prospects" is a cogent and particularly timely essay given the deterioration of U.S.-Soviet relations during the Reagan years. George examines previous efforts, particularly during the Nixon administration, and makes specific positive suggestions for improving the level of interaction. The essay by Barton Bernstein, "The H-Bomb Decisions: Were They Inevitable?", is a tightly written historical analysis of the decision process within and outside the Truman administration that led to the president's crucial January 31, 1950 announcement that America would build an H-bomb.

Michael Nacht's short essay, "On Memories, Interests and Foreign Policy: Vietnam," is especially interesting. Nacht "explores . . . the way images of international politics are formed" and "reviews the principal elements of the American world view that . . . led naturally and easily to the American military intervention in Vietnam." He concludes with an "assessment of contemporary American images of international politics in the wake of the Vietnam experience." Nacht's "central question" is "the effect of the Vietnam War on the American self image and on the U.S. role in the world" (pp. 359-361).

Nacht uses the well-known ideas of Thomas Kuhn on paradigm shifts in scientific thought and Leon Festinger's on cognitive dissonance to explain how the dominant cold war (containment) paradigm, formed in the immediate post-World War II period, was challenged by discrepant information and developments in the Vietnam War. The cognitive dissonance of the Vietnam War led key political players at home and abroad to challenge the containment paradigm. The result, according to Nacht, is that since the Vietnam War three distinct groups have emerged: paradigm defenders, paradigm adjusters, and paradigm deniers (pp. 373-375).

In summary, this volume has a few bright spots sprinkled among work of marginal quality and relevance to the editors stated purposes for the volume as a whole.

CHARLES M. BENJAMIN

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International Environmental Policy: Emergence and Dimensions. By Lynton Keith Caldwell. (Durham, N.C.: Duke University Press, 1984. Pp. xv + 367. $37.50, cloth; $14.75, paper.)

Lynton Keith Caldwell's book is an ambitious undertaking, setting before the reader a "survey of the worldwide movement for protection of the human environment" and promising to aid the reader to "fully understand the significance of international environmental policy . . . [by exploring] its origins and evolution . . ." (p. 3). The volume is a fastidious recounting of the names of international organizations and their chronology, goals, and program outlines. Information relating to formation of the organizations is introduced chronologically in the book's early chapters; the mid-sections are a collection of information presented by structure of organization and arrangement such as international law and regional activities; and the later chapters briefly acquaint the reader with the key issue areas in international environmental policy and international activity relating to them. Coverage of policy areas includes a broad range of topics stretching from the environmental problems associated with the North-South divide and resource exploitation, energy and environment, economic development, endangered species, wetlands, grasslands, national parks, urban settlements, and cultural heritage to problems of the "international commons" such as ozone depletion, carbon dioxide balance, and the dilemmas following from human exploration and presence in outer space.

However there are several difficulties that limit the volume's usefulness. One is the absence of focus in the description of international activities and the resultant feeling while reading parts of the book that one is plodding through brief annotated lists of issues and arrangements. Although Caldwell does state at the outset that "the far-reaching scope of the subject . . . precludes analytic treatment of most policy issues" (p. 3), even a descriptive work, as long as its stated purpose is to describe the *emergence* of an international movement and the effectiveness of international action to which it has evolved, should provide some guidance in distinguishing between successful and unsuccessful international activities and examine at least cursorily why some organizations and treaties are working while others are not. Thus, where Caldwell's description of implementation of the U.S. Marine Mammal Protection Act suggests that a specific type of market force is the crucial factor in the act's success, the author leaves it to the reader to infer the point and provide his own comparison with other activities that are discussed in the book (p. 246). For the same reason, after completing the volume one is not able to clearly distinguish an organization such as the International Maritime Organization, which has been regulating sea traffic for years, from the United Nations Environmental Program (UNEP), which has generated piles of paper and less action.

Finally, there is room for questioning the real significance of the documents upon which Caldwell bases the conclusions scattered throughout the descriptive material. Concerning the founding of UNEP in Stockholm, Caldwell states that "the improbable outcome of Stockholm was the extent of international consensus on the broad—but often detailed—provisions of the Action Plan" (p. 55), a statement of goals. However, one might as easily derive the opposite conclusion, that is, that broad consensus existed because environment was the equivalent of an "apple-pie or motherhood" issue in the United States. At other points Caldwell makes assertions about accomplishments of UNEP, but relies on a UNEP report, *The Environment in 1982*, to demonstrate them (p. 76) in spite of the fact that international organizations of this type routinely acclaim that their work has been "accelerated and intensified" and rarely publish self-critical assessments.

Caldwell's conclusion emphasizes the potential of nongovernmental organizations (NGO) for continuing and achieving the defense of earth, because governmental action is constrained by the East-West and North-South splits. The idea has merit as long as one keeps in mind two well-recognized points: NGOs from some parts of the world are government organs, and public interest groups cannot boast equal influence worldwide.

PAULETTE MANDELBAUM

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Congress and National Defense: The Politics of the Unthinkable. By Werner J. Feld and John K. Wildgen. (New York: Praeger Publishers, 1985. Pp. xiii + 127. $25.95.)

Congress and National Defense, despite its title, is not about Congress. Rather, it is a quick survey of defense and major foreign policy issues. The connection to Congress comes through the mouths of several senators and representatives quoted by the authors. Even at that, the handful of congressmen are hardly typical. Or perhaps they are—readers will never know how they were chosen. The authors merely claim that these members "show signs of truly strategic thinking" (p. v).

The only chapter dealing explicitly and consistently with Congress is chapter 2, "Congressional Organization and Process Regarding Defense Issues." Unfortunately, readers will learn little about how congressional organization and process affect defense policymaking. We get, rather out of the blue, the observation that: "Foreign policy decision-making analysis suggests that various clusters of variables must be taken into consideration to determine the effective cause or causes of particular policies. These clusters are institutional, idiosyncratic, societal, and international variables" (p. 37). Little more is provided beyond several implicit and explicit references to the role of interest groups and the military-industrial complex and a quick once-over of the Ripley and Franklin typology of crisis, strategic, and structural defense policy. We do learn, later, that "Congressional thinking about the MX is not Congress at its most sophisticated" (p. 72). The problem is that the evidence for the conclusion is little more than a series of speeches taken from a 1983 Senate debate. In short, if one wants to learn anything about the role of Congress in defense policy, the advice must be to look elsewhere.

On the other hand, if one seeks a generally informative but thin review of most issues of contemporary defense policy, this is an easy enough book to read. Chapter 1 presents a brief history of military and strategic issues. Chapter 3 covers strategic weapons, noting that for some congressmen the basis for opposing such systems as the MX is "not essentially military, especially not strategic" (p. 54). Chapter 4 covers NATO, while the fifth deals with nuclear arms control picture.

Except to learn what Feld and Wildgen think about these defense issues (they seem to adopt a traditional, somewhat conservative but level-headed view, and they seem critical of most congressmen), few readers will come away from this volume with a greatly enhanced understanding of either defense policy or Congress. The book does not deliver on the promise of its title; nor does it provide a thorough treatment of the military. There certainly is nothing inherently foolish or misleading in the book, but there simply isn't enough there to warrant spending the time required to digest it.

JACK JOHANNES

Marquette University

The Special Relationship between West Germany and Israel. By Lily Gardner Feldman. (Winchester, Mass.: Allen & Unwin, 1984. Pp. xi + 330. $35.00.)

An intriguing and well-documented tale of the complex and tragic German-Jewish/Israeli relationship, this work also affords the opportunity to analyze and evaluate the overused concept of "special relationships" in international affairs. Although the tale is well told, the concept remains perplexing and perhaps misleading.

The furor over a recent U.S. presidential visit to the Bitburg cemetery in the Federal Republic was merely the latest festering of the still unhealed wounds of German's past. Chancellor Kohl's Middle Eastern travels of 1984 also gave rise to controversy over the proposed sale of sophisticated weapons to Saudi Arabia. While in Israel, Kohl maintained that 40 years had produced a "new Germany" led by guilt-free leaders and entitled to the prerogatives of a major regional power; the Israeli government in effect said, "Not yet."

The cards remaining in Israeli hands—to be played for influence at such times of disagreement with Bonn—reflect what Feldman terms the continued special Bonn-Tel Aviv relationship, a relationship born of twentieth-century German anti-semitism and the Holocaust, and forged by political and economic sensitivities and needs on both sides. However the pace of evolution, means of influence, and issues of contention now evident in this relationship do not appear to differ markedly from those of any bilateral international diplomacy. Therefore, one wonders what is gained by viewing Israeli-West German relations as "special." As Anthony Eden discovered to his chagrin at Suez, belief in supposedly special ties (e.g., U.S.-UK) can produce the gravest sorts of miscalculations and delusions.

Feldman has done an excellent job of putting Federal German and Israeli relations in political and economic context. These two countries, vying for legitimacy and economic stability, in part have used each other since the early 1950s to help satisfy these needs. The Federal Republic sought expiation of guilt and full acceptance by the Western world. Obversely, Israel sought immediate financial stabilization and access both to the Western world and weapons.

However, the very predominance of pragmatic political motives over moral imperatives (p. 60) belies the special relationship. Feldman identifies the essence of such a relationship as psychological: "The historical and intense features of a special relationship are transitory. The psychological relationship that emerges from them, however, must endure if the special relationship is to endure" (p. 11). The psychological scars in Israeli-German politics are still very pronounced and can indeed embarrass a government into changing or postponing important plans. The material foundation of relations between the two states has changed and is changing still. The Federal Republic has found basic legitimacy and diplomatic integration in the European Community—complete with its efforts to foster "special relations" with Arab states (p. 203). Israel too has gained acceptance, even now by the first Arab state, and broadened its economic ties.

Therefore, although Israelis do not necessarily forgive and forget, some of the suspicion, hatred, and fascination has receded from Israeli-German relations. The intensity of psychological involvement with the former oppressor may have eased even as economic and security involvements and tourist travel have increased.

Germany offered atonement to Israel mainly as a means to larger political ends, ends which could have been achieved otherwise; Israel chose to respond to German overtures as a means to larger economic ends, ends which also could have been achieved otherwise. If there were something abidingly special in this, one would expect at least some trace of similar imperatives in Israeli-East German relations, a trace which evidently does not exist.

FREDERIC S. PEARSON

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The Public and Atlantic Defense. Edited by Gregory Flynn and Hans Rattinger. (Totowa, N.J.: Rowman & Allanheld, Publishers, 1985. Pp. xviii + 398. $45.00.)

Although NATO's *raison d'être* of deterring military attack against Western Europe has been realized for 36 years, it has been plagued by dissensus among governments and political elites almost since its inception. The editors of this volume believe (correctly, I think) that neither the state of available public opinion data sets nor existing analyses permit informed, in-depth assessments of the substance and import of mass attitudes for Alliance relations. They identify the book's central purpose as the presentation of mass or general opinion data on security policy "from all available sources" that will add up to a "general inventory" and the construction of "a more adequate profile of the patterns of popular support and of popular discontent that Western governments must confront . . ." (p. 1). In the brief introduction they list "four analytical axes" they believe represent the "core element[s] in public perceptions of Western security and security requirements . . . images of the Soviet Union, . . . of security, . . . of deterrence, . . . and of allies" (pp. 7-8).

Even if the contributors of the seven country chapters (six European members of NATO and the United States) had achieved these goals, the book would have lacked cogency and utility for effective country-by-country analyses, and especially for generalization to the level of the Alliance and for comparison. But in four or five of the country studies the stated goals are hardly approached, in part because of the purposeful avoidance of considerations "of elites, of opinion leaders, of peace movements, mass-elite, elite-media *(sic),* and other interactions that shape the process of political communication and the formation of political will [and] . . . ongoing political battles" (p. 9). Most of the contributions suffer from poor, often inchoate data presentations and analyses, and there are numerous factual errors and unwarranted inferences. One may wonder how an understanding of the public attitudes that "Western governments must confront" could be attained without considering mass-elite relations, political communication, the relative political importance of public opinion in the policy formulation process, and other factors expressly avoided.

The four "analytical axes" serve more as rubrics for data presentation than as analytic levers. Most of the chapters overflow with data, sacrificing coherence and clarity to volume. The more than 200 tables (many running to more than one page, one to four and one-half pages) and the (literally) thousands of other numbers that fragment the prose doubtless contain raw material for the inventory intended. But the data organization tends to hamper the development of sharp, integrated analytic focuses, and the attempts to explain, explain away, or apologize for the data dull the effectiveness and utility for would-be users of what is frequently poorly contextualized and (perhaps unavoidably) dated material. No serious effort is made toward cross-national comparability, either in presentation or analysis. Most limiting are the injunctions imposed on the contributors regarding explicit considerations of the sorts and degrees of importance mass attitudes might have on policymakers, the opportunities for exploiting the (predictable) indwelling inchoateness and intransitivity of much of public opinion, and the relative abilities of elites and policy influentials to guide or manipulate it. Dynamic interactive analytic considerations are sorely lacking.

Working at the book allows the reader to learn useful things about some of the countries. In particular, the chapter on the United States, by William Schneider—and, to a lesser degree, those on the Federal Republic, Italy, and Norway by Hans Rattinger, Sergio Rossi, and Ragnar Waldahl respectively—yields some useful insights. Where the authors transcend the constraints noted previously, they can identify dynamic relationships between important substantive, reasonably current issues and mass opinion. The other chapters often seem more like efforts to explain whatever data could be found and to lament what is not available than attempts at policy-relevant analysis. The treatment of France is most distressing in this regard. In general, informative and useful presentations and interpretations appear in what seems an inverse relationship to adherence to the common analytic dimensions and constraints stipulated in the introduction.

The country-by-country and summary findings are hardly startling. Thus, the publics of at least these NATO states see superpower confrontation to be as great or a greater threat to security than the Soviet Union per se. They favor collective defense through the Alliance as a general proposition, but do not like paying some of the political and economic costs that may entail. And, finally, few correlations exist (except party identification in some countries) between such sorting dimensions of publics as age, education, and socioeconomic status on the one hand and attitudes on particular security issues on the other.

In sum, the results do not seem commensurate with the obviously great effort invested in this enterprise. The book is satisfactory neither as an inventory nor in analytic terms. More might have been achieved with less.

MARTIN O. HEISLER

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Contending with Kennan: Toward a Philosophy of American Power. By Barton Gellman. (New York: Praeger Publishers, 1984. Pp. xvii + 172. $21.95.)

In *Contending with Kennan,* Barton Gellman pursues a thoughtful analysis of George F. Kennan's written works as they relate to the use and misuse of American power in world affairs. Through this analysis, one gains substantial insight of Kennan as a theorist, historian, diplomat, and elitist.

Intended as an analytical biography, Gellman sketches Kennan as a political statesman, one who genuinely toils over how decisions of state affect humanity in general and the American national interest in particular. Kennan transcends partisanship yet is constantly embroiled in the reality that partisan pressures dictate America's world diplomacy. Kennan subscribes to what Gellman dubs the snowball theory of history; that all great issues are more a matter of workday choices than greatly planned thought. World affairs, then, are an incrementalist game, intrinsically bound by partisan parameters.

Crucial to Kennan's view of America's role in world affairs is first, his notion of America's power relationship within the international community and the responsibilities that emanate therein and second, the need for America to protect its national interest. These two preconditions necessitate that America protect its security in order to advance its general welfare. Accomplishing this will ultimately advance international stability and peace. Gellman portrays Kennan here much the way Louis Hartz portrays American political culture in his classic work, *The Liberal Tradition in America* (Harcourt, Brace, Janovich, 1962).

Kennan sees as fundamental that America create alliances with key nations and pursue policies that protect strategically important international areas such that a balance of power would result among conflicting nations. Ultimately, America must concentrate its efforts in industrialized Europe and retrench elsewhere where the returns would be marginal. America, then, must recognize that the most resourceful manner to solve world problems is by creating a stable environment for itself and then allowing the benefits of that stability to slowly permeate into the more depressed world regions.

How can Kennan morally justify this inherently selfish approach to world affairs? Ultimately, the justification is synthesized in the proposition that what benefits America will ultimately benefit the world, at least the noncommunist world.

Although one can accurately depict Kennan as hawkish (he would advocate exercising military presence where it would keep foolish, would-be anti-American aggressors at bay), he deplores the dangers of the nuclear age. It is dangerous from a political standpoint because nuclear threats are, in reality, merely idle threats. Yet a succession of idle threats would likely render world order uncontrollable. In the final analysis, realistic containment necessitates a rational deployment of psychological, economic, political, and occasionally conventional military pressure to show what Kennan terms quiet strength. By strategically utilizing quiet strength, America can adjust its posture to international turmoil as it takes new forms.

Perhaps more than any other problem, Kennan laments that the most substantial threat to a rational approach to world affairs results from America's system of representative government. He argues that the electorate is too ignorant to grasp the proper comprehensive posture of America's role in world affairs. Juxtaposed with this proposition in his view that government is too gutless to eschew transient public opinion and too selfish to admit that a solid core of intellectual, statesmanlike elite are needed in government to govern for the good of both the United States and the Free World.

Barton Gellman should be commended for this insightful, often critical, compact analysis of George Kennan. Gellman's level of intellectual sophistication defies his extreme youth. *Contending with Kennan* brilliantly shows the value of the traditional approach to political science and is without question a valuable addition about the perspectives of those who helped shape twentieth-century American foreign policy.

KENNETH PAUL NUGER

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Conventional Deterrence: Alternatives for European Defense. Edited by James R. Golden et al. (Lexington, Mass.: D.C. Heath, 1984. Pp. ix + 245. $27.00.)

The recent debate over the deployment of intermediate-range nuclear forces in Europe has given rise to a variety of proposals for NATO moving away from a reliance on nuclear weapons to defend Western Europe. Many of these proposals have focused on the improvement of NATO's conventional capabilities. Such a development, it has been argued, could raise the nuclear threshold and thereby provide a rationale for the repudiation of the policy of "first-use" of nuclear weapons.

To be effective, however, this enhanced conventional strength would have to serve not merely as a defense, but as a deterrent force as well—and therein lies the problem. We have come to think of deterrence as fundamentally linked to the unacceptable damage delivered by nuclear weapons. Can nonnuclear forces provide that function? And, if so, what are the prospects for moving the Atlantic Alliance in that direction? These are the critical questions dealt with in this timely collection of essays by 18 different specialists, most of whom are scholars affiliated with West Point and the U.S. Army.

First, the title is somewhat misleading. Essentially, the essays discuss the entire range of issues related to the role of conventional forces in European defense. Most of the essays do not explicitly relate their analyses to the vague concept of "conventional deterrence," a concept not clearly defined until chapter 5. This is not to argue that the collection is weakened, but one should not expect full enlightenment on the concept of conventional deterrence. Instead, the essays assess the military, political, and economic problems associated with the suggestions for a greater NATO reliance on conventional weapons.

The linkage between conventional capability and nuclear deterrence is a little-explored area, and this book sheds some light on the problem. Repeatedly, the authors reaffirm the viability of the strategy of Flexible Response as an approach to deterrence that best connects conventional and nuclear weapons. In fact, the book's overall impression is that proposals for conventional deterrence that do not include the nuclear option would not be credible. Unlinking nuclear weapons from NATO's defense would not, it contends, enhance deterrence.

Conventional deterrence schemes, according to these essays, tend to be politically unacceptable because of their implied war-fighting scenarios. They are economically unacceptable because of the high costs. They are technologically unfeasible because of the lack of appropriate weapons. And, they may be strategically unacceptable because of the prospect of decoupling nuclear weapons from European defense. We are reminded of the national variations in defining the strategic requirements of NATO—and that there is little sign of a willingness to adopt a new doctrine, one less subject to interpretation than Flexible Response. Moreover, to be effective, a conventional deterrent would require a greater degree of intra-alliance weapons standardization and procurement cooperation than currently exists.

It was refreshing to discover at several points in the book a critique of the Army's drift toward the new Air-Land Battle doctrine. If defense—or deterrence—is the goal of NATO, such attack-oriented plans may not be very helpful in reassuring the Soviet Union of those intentions.

Although this is a well-informed, clearly presented collection on an important strategic and defense policy question, the authors have a tendency at times to get lost in tactical issues and to lose sight of the overall theme. The editors, though, pull the issue together well in the end.

As attractive as a conventional deterrence for Europe may appear, as these essays demonstrate, such a strategy may be impossible to implement and, in fact, could create more problems than it solves. The final conclusion of this book is that deterrence in Europe requires a continued reliance on nuclear weapons and adherence to the broad outlines of Flexible Response—including first-use.

HAROLD MOLINEU

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Space Commerce: Free Enterprise on the High Frontier. By Nathan C. Goldman. (Cambridge, Mass.: Ballinger Publishing, 1985. Pp. xiii + 186. $25.00.)

The subject of space to most political scientists specializing in international relations suggests two things: the militarization/national security issue and the issue of national sovereignty/international regulations.

The book under review, however, is not on the issue of militarization of space; as the title indicates, the book is on business enterprises in space. Nathan Goldman concludes that space will be important for the national economy and commerce. He argues that the government must create a Department of Space in order to define and regulate national and private interests. He warns that policymakers must steer clear of overregulation that will stifle private commercial development in space, and argues that private enterprise must assert itself for a greater involvement in space commerce.

A large portion of the book is devoted to identifying technological developments and to explaining commercial activities and feasibilities within the scope of Goldman's model of space commerce. His model is based on space transportation, which is comprised of three commercial components: communication, remote sensing, and manufacturing. He defines space commerce as "the market for space goods and services including space transportation, communications, and remote sensing satellites; manufacturing in space; and eventually the transmission of solar energy back to earth by satellite and the mining of celestial bodies" (p. 15).

Goldman points out that by the early 1980s the U.S. government's expenditures on the military in space were far greater than the civilian programs. He demonstrates that international competition in space is rapidly increasing, not only from the Soviet Union but also from the European Space Agency (ESA), Japan, China, Italy, and India. American leadership in this competition, he believes, will reward the nation both politically and economically.

As a result of some international regulations and commercial competition in space, the United States faces some conflicts between its foreign policy and domestic policy. Although Goldman does not analyze this conflict, he cites the case of Intelsat. "For instance," he states, "if the United States suggests a worldwide solar energy consortium or an organization like Intelsat for the Western Hemisphere . . . potential members will note the unreliability of the United States in its Intelsat agreements and be very leery of such arrangements" (p. 139). Furthermore, he sees the commercial issue of space creating certain confrontations between the major democratic space powers, the third-world nations, and countries of the Eastern bloc.

Although *Space Commerce* does not analyze any issue directly related to international politics, it does provide good background information. It would be useful for political scientists to pursue some of the serious scholarly endeavors in this emerging frontier of social science knowledge, for example, the nature and processes of the emerging international regimes in space, their interrelationships, national and international policy processes and their possible consequences, and the concept of "life-boat" ethics.

ROBERT S. JUNN

Grand Valley State College

Closing the Gold Window: Domestic Politics and the End of Bretton Woods. By Joanne Gowa. (Ithaca, N.Y.: Cornell University Press, 1983. Pp. 208. $27.50.)

The international economic position of the United States has rarely been as exposed as it was during the summer of 1971. The nation was developing its first balance of trade deficit of the twentieth century. International confidence in the American currency plummeted as dollars held abroad were converted into gold and foreign currencies at breakneck speed. The nation's gold stock could cover only one-eighth of foreign dollar holdings. To salvage the situation, President Nixon announced on August 15, 1971 that the dollar would no longer be convertible into gold. This decision "effectively brought to an end the Bretton Woods regime that had governed postwar international monetary relations" (p. 9). In seeking to account for this radical departure, Joanne Gowa highlights the role played by domestic political forces in the United States rather than looking to such international determinants as the growth of economic interdependence and decline in American political and economic hegemony. In the process, *Closing the Gold Window* combines an explicit treatment of fundamental theoretical issues in international political economy with well-grounded empirical research of a momentous political episode.

Although the book emphasizes domestic, unit-level determinants of U.S. foreign economic policy, Gowa acknowledges that *a* change in U.S. international monetary policy was dictated by international, systems-level developments in the late 60s: "As the character of the international politics changed and as the stock of dollars abroad exerted ever-greater strains on the Bretton Woods system, the ability of the United States to . . . preserve its freedom of action at home and abroad, deteriorated" (p. 173). By the summer of 1971, the external crisis ruled out both an effective reassertion of American control over the monetary regime and its major trading partners and the continuation of policies of inaction adopted since Nixon took office in 1969. The international situation left only two alternatives open for the administration. Either they could stabilize international monetary relations by curtailing domestic economic and foreign security policies that contributed to the payments deficit, or they could maintain autonomy in national policy by scrapping the monetary regime based on dollar convertibility.

Situational factors not only predicted a change in policy, but also can explain the exact timing of the change. The disturbed state of international financial markets and, more particularly, intense speculation against the dollar served as the immediate "thunderclap" precipitating the August 15th decision (p. 162).

Hence, situational factors predicted a policy change, limited the range of the policy options, and even determined the timing of the change. Most traditional international relations specialists would have been satisfied to pack up their academic bags at this point. But Gowa pushes on—reflecting the pursuit of new heights in scholarly inquiry.

In particular, the author pushes on to explain the administration's decision to sacrifice the Bretton Woods regime instead of compromising domestic economic and foreign policies. It is at this level of analysis that Gowa finds domestic political determinants most powerful as opposed to external pressures. The gold window is closed, Gowa argues, because a clear domestic political consensus existed that subordinated the preservation of the Bretton Woods regime to the maintenance of policy freedom in pursuing U.S. domestic economic objectives (e.g., monetary expansion) as well as general foreign policy objectives (e.g., continuing the Vietnam War).

Having decided to scuttle the postwar monetary regime, the administration still had some choice in how to construct a replacement. Gowa again makes the case that domestic political factors, and in this case, the structure of the international monetary policymaking process in the Nixon administration, determined the choice. One option was to enter into multilateral negotiations to reform the international regime (presumably by instituting some degree of limited exchange rate flexibility). A reformist, multilateral approach was, however, overruled by a conservative Treasury Department. Treasury, the preeminent institutional actor in the making of Nixon international monetary policy, was skeptical of reform efforts and flexible exchange rates. As a result, U.S. policy took on a unilateral, neomerchantalist cast. Our trading partners were, without prior consultation, "shocked" by the August 15th decision and were bullied to revalue their currencies and reverse trade surpluses with the United States.

Although Gowa's unit-level explanations of the specific content and style of U.S. policies are eminently plausible and well-documented, it must be pointed out that the administration's behavior clearly reflected America's unique *systemic* position in the world economy. The subordination of internationalist concerns to national objectives itself reflects the privileges of a nation enjoying a relatively closed economy as well as a dominant, if not still hegemonic, position in the international system. The ability to maintain national autonomy in face of external pressure as well as the administration's unilateralist behavior were clearly possible because of America's leading position in the hierarchy of nations. At the same time, the neomerchantist character of the August 15th action can be linked to the growing frustrations of a declining hegemon. To conclude that policies were made by domestic actors and served certain domestic interests (a not altogether surprising finding) does not make the case for unit-level analyses. Although Gowa explicitly acknowledges that both domestic and international determinants had a role to play in the closing of the gold window, it must also be recognized that the former are linked to if not often derived from the latter.

Even if a strong case can be made for the explanatory power of unit-level analyses in this instance, a favorable conclusion regarding the general utility of domestic explanations for the foreign economic policies of other countries cannot be made. Because of the nation's dominant and relatively insular position in the world economy, the United States is the one country whose policies are least subject to external constraints and most likely to be internally generated. No other advanced capitalist country enjoys these attributes. Hence the application of unit-level explanations to the U.S. may be the exception (albeit an important one) rather than the rule.

Although these issues of levels of analysis remain open, progress towards their resolution has been furthered because *Closing the Gold Window* confronts them head-on in an exemplary empirical study.

GLENN R. FONG

University of Illinois at Chicago

Latin America, Western Europe and the U.S.: Reevaluating the Atlantic Triangle. Edited by Wolf Grabendorff and Riordan Roett. (New York: Praeger Publishers, 1985. Pp. xxi + 295. $37.95.)

As Latin American countries have become more significant actors on the world scene in the context of "a slowing down of the capacity and the will of the United States to impose its world view on the international system" (pp. 276-277), their more variegated and complex foreign policies have drawn increased scholarly attention. This volume, a compilation of papers presented at conferences in 1981 and 1982, focuses on the often overlooked changing relationships across Latin American countries (especially Brazil and to a lesser extent Argentina and Mexico), Western Europe, and the United States. It represents a useful, solid contribution to this growing foreign policy field.

One appeal of the book is its international authorship, for it consists of an introduction and a conclusion and 14 chapters by three U.S., three German or Austrian, and eight Latin American authors. At the beginning and end and in their succinct individual chapters (as does Gerhard Drekonja in his contribution), the editors delineate some of the geopolitical, economic, cultural, and ideological dimensions of a new "Atlantic Triangle" without fully succeeding in transforming the term from an organizational tool into an analytically useful concept. Causes and indicators of a reemerging "silent partnership" (p. xiii) between Europe and Latin America include more extensive trade, financial, and investment ties. Also significant are growing arms sales (in 1980, almost one-half of such sales to South America came from Western Europe) and provision of nuclear technology by European countries to one or more of the larger Latin American countries. Part of the reason for an affinity between Western Europe and Latin America is that they are more similar to each other than either is to the United States in terms of the greater ideological nature of their political parties and in the more substantial role for the state in their economies and societies. Yet what emerges from discussions by such authors as Drekonja, Roberto Russell, and Juan

Carlos Puig is the sharp constraints to improving or changing the nature of the relations between Western Europe and Latin America. Financial and particularly commercial concessions by Europe are extremely unlikely. And as the editors underscore, the Falklands/Malvinas war in 1982 represents a setback, although it affected Latin American relations with the United States more than with Europe. In a contribution on security, Barros notes that even defense of the South Atlantic, which may be "the most relevant strategic issue" (p. 172) involving the three areas, does not attract a consensus for any well-defined triangular relationship.

A helpful historical overview by Stanley E. Hilton of Latin American-Western European political and strategic relations through 1945 and a more choppy review of their economic relations by Werner Baer underscore the once strong relationship between the two regions. Among the most interesting and useful essays in the book are those providing Latin American perspectives on debt (by Rosario Green) and energy (by Carlos Moneta). More disappointing are Albrecht's essay providing a "German" perspective on security issues and the three essays that focus specifically on Brazil (by Walder de Góes, Carlos Perez Llana, and Roberto Fendt, Jr.), for they do not go beyond very general arguments or well-known points about the decline in U.S.-Brazilian relations under Carter (because of disputes over human rights and nuclear technology) or the growing importance of economic factors in shaping current Brazilian foreign policy.

As is unfortunately the case in many edited volumes, the contributions are uneven, and not all topics are equally well covered. Nevertheless, this book can help sharpen our thinking about the possibilities for cooperation and conflict among the larger Latin American countries as their international importance continues to increase, the United States, and Western European countries.

JONATHAN HARTLYN

Vanderbilt University

U.S. Security Assistance: The Political Process. Edited by Ernest Graves and Steven A. Hildreth. (Lexington, Mass.: D.C. Heath, 1985. Pp. xii + 192. $24.00.)

This uneven collection of essays results from a study of attitudes toward U.S. security assistance undertaken in early 1983 by the Center for Strategic and International Studies at Georgetown University. The Preface indicates that the CSIS study received important help from several Reagan administration officials including Undersecretary of Defense for Policy Fred Iklé and Lt. Gen. Philip Gast, director of the Defense Security Assistance Agency. James R. Schlesinger also played a key role in the enterprise and wrote the introduction to this volumee.

The editors have gathered four substantive essays plus a fifth, by Ernest Graves, that summarizes the others and makes several recommendations to improve executive-congressional relations in the area of security assistance. These chapters cohere rather nicely: the first offers an historical overview of U.S. military assistance programs, the second traces congressional and media attitudes toward these programs since 1959, the third describes the roles played by the Executive and Congress in formulating and funding this assistance, and the fourth looks at public and elite attitudes toward these programs since the fragmentation of the cold war consensus. Unfortunately the quality of these essays varies considerably.

Richard F. Grimmett, in "The Role of Security Assistance in Historical Perspective," provides an exceedingly bland court history of these programs based on his opening assertion that "the overall goal of U.S. defense and foreign policy since World War II has been to foster a stable, peaceful world conducive to national security, economic prosperity, and individual freedom" (p. 1). Yet within these narrow confines the essay does contain very useful data on the evolution, changing focus, and variable size of these security assistance programs. We learn, for example, that the $3.5 billion appropriated for them for FY1982 represented a historic low in comparison to the overall budget authority of the Defense Department, which for FY1982 was $214 billion, and that foreign military sales credit appropriations have been by far the most volatile part of the assistance programs, with wild fluctuations since 1969. But Grimmett's conclusions are unexceptional: "the levels of the programs have risen and fallen in relation to major international crises perceived by U.S. policymakers to require use of the program to serve important national security interests" (pp. 38-39).

Steven A. Hildreth's "Perceptions of U.S. Security Assistance, 1959-1983: The Public Record" is clearly the weakest essay in the collection. It seeks to recount the arguments advanced by presidents for security assistance and to indicate congressional and press reactions to these initiatives. For reasons never made clear, Hildreth begins his essay in 1959 instead of a decade earlier, and presents it in a most haphazard manner, peppering the reader with largely random snippets of congressional testimony and editorial comments. This highly episodic chapter concludes with findings that will surprise few: Presidents have offered the same kinds of rationales for security assistance since 1959, Congress has used many of the same arguments pro and con year after year but has generally become less supportive of these programs, and the media have become more sophisticated in their treatment of the issue as their interest in it has grown.

Far superior is Franklin D. Kramer's "The Government's Approach to Security Assistance Decisions." Kramer provides a lucid account of the budgetary process that governs security assistance. Beginning with the claim that OMB recommendations for this program are largely based on the previous year's funding levels, he carefully describes and assesses the roles played by other executive actors and by the relevant congressional committees and concludes with an extremely insightful analysis of executive-congressional interaction in which he rightly emphasizes the highly political and symbolic nature of security assistance.

In "The Domestic Constituencies of the Security Assistance Program," Robert B. Mahoney, Jr. and David L. Wallace rely heavily on polling data to measure support since the early 1970s for security assistance. Employing the findings of James Rosenau and Ole Holsti in, for example, "Vietnam, Consensus, and the Belief Systems of American Leaders" (*World Politics, 32,* 1979, 1-56) and the foreign affairs surveys sponsored by the Chicago Council on Foreign Relations, Mahoney and Wallace focus on the attitudes of the general public, opinion leaders, and interest groups toward military aid. They conclude that in the absence of a domestic foreign policy consensus there has been little public support for security assistance, whereas only the cold war or conservative internationalists continue to defend these programs among elites. To make security more palatable the authors recommend that it be firmly joined to economic aid and that Congress be provided with more and earlier opportunities to contribute to the budgetary process.

The fundamental weakness of this volume lies in its refusal to acknowledge and defend its basic premise: that U.S. security assistance has been and continues to be a positive good. Most of the authors are primarily concerned with devising strategies to "sell" military assistance programs in an era of foreign policy dissensus. As a result this book (the Kramer essay excepted) will be of more interest to Washington "insiders" than to scholars.

RICHARD A. MELANSON

Kenyon College

Between Dependency and Autonomy: India's Experience with the International Computer Industry. By Joseph M. Grieco. (Berkeley: University of California Press, 1984. Pp. 224. $37.50.)

Joseph Grieco's study of the Indian computer industry follows the work of Peter Evans, Gary Gereffi, and Richard Newfarmer in analyzing the political dynamics of foreign direct investment in third-world manufacturing through close sectoral analysis. Arguing for the superiority of a "bargaining model" of host-investor relations, Grieco departs from previous studies by challenging dependency expectations. He argues that changes in the international structure of an industry can provide opportunities as well as constraints for developing countries, *if* host countries can organize effectively to exploit them.

The debate hinges in no small measure on the indicators of LDC success. Bargaining theorists view changes in contract terms, increased local equity participation and technological upgrading as *prima facie* evidence of an improved LDC position. Dependency theorists tend to weigh performance against a more "autonomous" counterfactual. Hewing more closely to the first line, Grieco tracks India's progress against the benchmark of its own stated goals, devising useful independent measures of technological progress as well as national control and market structure.

The argument for expanded national opportunities stems from the logic of the product cycle and technological change. The introduction of inexpensive and flexible new technologies, particularly the mini- and microcomputer, was paralleled by an increased level of international competition. The changed market conditions associated with lower barriers to entry eroded the bargaining power of IBM, which initially occupied a dominant position in the Indian market.

India could not replicate the full range of IBM's technological and service capabilities. A state-owned national champion proved a less than wholly satisfactory substitute. Nor could India force IBM to make concessions on local equity participation. Rather than dilute control in the face of escalating demands, IBM chose to withdraw from India, an outcome Grieco sees as a major, although not insurmountable setback for the achievement of India's computer goals. Despite these limitations on India's power and freedom of maneuver, the availability of new technologies permitted a changed strategy built around independent Indian systems engineering and a reduced reliance on mainframes.

This is all straightforward. As Grieco correctly argues, however, changes in the international industry are only a backdrop to an understanding of national policy. More interesting is the discussion of the domestic political and organizational bases of India's computer strategy. The greatest theoretical failure of the dependency approach was never its insistence on the significance of external constraints, but in the ad hoc treatment of domestic political factors. This is Grieco's strong suit, but the potential for a broader theoretical contribution is diluted somewhat by the stale contrast between dependency and bargaining models.

Grieco shows nicely how the state's capacity to devise and implement an effective strategy hinged on political conflicts within and outside the Indian government. Intrabureaucratic battles for control of policy and attendant resources initially undermined the cohesion of national policy. Centralization solved that problem, but when the state-owned enterprise proved unable to deliver on promises made, private national firms mobilized both politically and in the market to secure an expanded role for themselves. Exploiting economic nationalism, the new entrants forced a reorientation of national strategy.

Neither dependency nor bargaining approaches, operating at the wrong level of analysis, are equipped to explain these national-level outcomes. Grieco's study—and the case studies in Peter Evans's *Dependent Development* (Princeton University Press, 1978) for that matter—demonstrate the primacy of internal political variables, but don't present, or even work from, theories of national policy making. On balance, I find Grieco's demonstration of increasing LDC power more convincing than Evans's insistence on the determinacy of international constraints. But neither approach confronts the central theoretical question. *Given* certain international constraints associated with the nature of the multinational enterprise, some states organize more effectively than others in challenging them. Why? An international-level explanation is only convincing when it can be shown that similarly situated states respond similarly to external constraints. Since this is not true, more systematic attention needs to be focused on the characteristics of the state, the power of different social groups, ideology: in short, on the determinants of public policy.

Overall, Grieco has written a solid study, made particularly interesting by his choice of a "hard" case in which technology is changing rapidly. The argument is tightly knit and thoroughly documented. For those interested particularly in multinationals, there are nuanced discussions of the implications of different trade controls, export processing zones, and different strategies for affecting linkages with local industries.

STEPHAN HAGGARD

Harvard University

The New International Economic Order: Conflict and Cooperation in North-South Economic Relations, 1974-1977. By Jeffrey A. Hart. (New York: St. Martin's Press, 1983. Pp. xvii + 180. $22.50.)

Eschewing partisanship and pure description, *The New International Economic Order* (NIEO) attempts to explain the changes in international regimes governing North-South economic relations in the period from 1974 to 1977 (Part 1) as well as the differences on this issue *within* both the developing and developed country blocs (Part 2). By emphasizing explanation, Hart provides fresh insight into a topic that is much discussed and hotly debated.

In his explanation of both regime change and foreign economic policy in individual countries, Hart draws upon factors from all three levels of analysis. First, international power—defined as capabilities relevant to North-South relations—is found to provide a basic although partial explanation of regime change and policy in the monetary and foreign investment issue areas. It does less well as an explanation in the area of commodity trade. Yet by applying concepts of power to the study of North-South relations, Hart clearly distinguishes himself from many scholars working in this area and grounds himself within the realist school of international relations, an approach that in the past has said little on the calls for a NIEO except in a rather disparaging way. On these grounds, the book will certainly be a contribution.

Second, Hart finds that "interests," a national level concept "largely rooted in [the] Marxist tradition" (p. 75), account for much of the variance within his sample of 14 Latin American and Caribbean countries and the members of the Organization for Economic Cooperation and Development. The level of development is important in explaining the salience of issues within the former group, whereas size is central in accounting for the split between the more and less forthcoming members of the latter. Third, cognition or individual world view is of varying importance across the cases. Hart does not draw any general conclusions about its explanatory power.

The New International Economic Order is part of a growing genre that seeks to test several competing and sometimes complementary theoretical approaches. It does so better than most. Yet Hart does not present an integrated theory of international relations, but rather three concepts whose acknowledged interrelationships are never clearly defined. As a result, the more general arguments that he might have addressed to the field of international relations as a whole are not developed.

The book's greatest strength is its extremely careful attention to constructing indicators for often ambiguous concepts and addressing potential counterarguments. On this score, the book could serve as a model from which many could profitably draw. This strength is also the book's principal source of weakness however, because the criteria for coding the often ordinal level variables are not always made explicit and therefore open to questionable interpretations. Is the commitment of the developed countries to negotiate a common fund for commodities really a case of greater regime change as Hart asserts, rather than the actual adoption of an albeit modest generalized system of preferences by the United States, the European Economic Community and others? Additionally, the discussion in the text focuses primarily on the presentation of the data on the many indicators. It tends, as a result, to be mechanistic and sterile. The reader misses much of the political drama associated with the negotiations for a NIEO, and the neophyte would perhaps come away lacking a good grasp of the very real issues and interests at stake.

Despite these shortcomings, *The New International Economic Order* is a solid analysis of the various international positions taken on a highly contentious issue. It presents original research on the foreign economic policies of both the developing and developed countries. And, most important, it moves us significantly closer to an understanding of the—at best—mixed progress toward a new international economic order.

DAVID A. LAKE

University of California, Los Angeles

NATO Arms Co-operation: A Study in Economics and Politics. By Keith Hartley. (Boston: George Allen & Unwin, 1983. Pp. 228. $35.00.)

The standardization of armament production hass both a political and economic side. Keith Hartley has much to offer in the way of ordering a set of problems associated with weapons standardization and a review of previous findings delineating the prospective savings in defense production. He is especially critical of the absence of conceptually clear empirical analysis of the prospective cost savings of standardization. Although the book is full of big and little questions, the central paradox of the study addresses the problem of why so little arms cooperation takes place when the cost savings seem so substantial.

The book is divided into two parts. The first is a rather complete treatment of the economic issues involved in the benefits of standardization. Although Hartley strives to balance economic arguments with political factors, discussion of the issues revolves around the economic concepts and analyses. The introduction of a notion of the "political market place" is welcome, but far too simple to provide a well-rounded analysis. All too frequently, politics emerges as the residual basket of factors that muddles both efficient production and economic analysis. This should not be regarded as a limitation to the book, however, because the economic analyses are thorough, and they quite accessibly present findings from the more technical studies of cost savings. In particular, Hartley concludes that the combination of savings from economies of scale, learning economies, and free-trade effects would produce a lower-bound estimate of a 20% unit cost saving from NATO standardization in weapons acquisition.

Part 2 presents a more detailed study of weapons standardization in the field of aerospace. Five chapters present the results of 64 interviews in 20 U.S. and European aerospace companies between 1977 and 1980. The state of the aerospace industry is described, and it is revealed that national industries overwhelm the prospects for standardization. Hartley concludes that budget maximizing bureaucracies favor expensive weapons to raise agency budget shares. Producers regard overseas purchases as least profitable. With regard to work sharing or licensing of production, the picture is confusing, and the costs seem to outweigh the benefits. Major attention is given to European joint production ventures, which turn out to be most attractive to industry because they are believed to be more difficult to cancel, and cost escalation is at least as low as for national programs despite the longer time in development.

The book concludes with a few very short notes on directions for improving competition in NATO weapons markets. Hartley is also generous in supplying quite complete results from his interview questionnaire. Despite a NATO-wide orientation, the book is best on UK arms procurement and experience with standardization. Also, one wishes that interviews of government procurement officials had been included to better balance the analysis. In the end, Hartley has provided a solid contribution on an understudied but important topic.

WILLIAM K. DOMKE

University of California, Davis

Arms Production in Developing Countries: An Analysis of Decision-making. Edited by James Everett Katz. (Lexington, Mass.: D.C. Heath, 1984. Pp. xii + 371. $30.00.)

This book deals with a growingly important phenomenon, the production of weapons in developing countries. It correctly notes that although vast amounts of scholarly analysis have been offered on arms transfers from developed to developing countries, very little attention has been given to arms production programs within the Third World. The volume fills the void rather effectively by dealing (in a series of 14 chapters) with the arms production programs of all the important third-world self-producers, as well as by including three more general chapters on the phenomenon in question.

The case studies are similar in focus: They assess the motives behind the decision to establish weapon production programs, include historical description of the development of these programs, and evaluate the political and economic consequences of the various programs. Although virtually all chapters include useful bibliographical references that cannot be found elsewhere, some case studies tend to be chronological rather than analytical in nature.

The production programs of three Latin American countries are analyzed. The chapter on Argentina dwells on the impact of the Falklands/Malvinas war on Argentina's program, concluding that the war left the defense production on a similar level to what it was before hostilities. The chapter on Brazil examines the trend toward the civilization and privatization of the arms industry and predicts that Brazil will adopt a military industrial complex model rather than the state ownership model (which dominates in most countries). The case of Mexico is concluded with cautious prediction, with the author emphasizing the economic hardships of the country as a major factor in determining its arms production success.

Two Middle Eastern countries, Egypt and Israel, are covered in the volume. The case of Egypt dwells on the formidable obstacles facing a developing country in its effort to build a weapons program; it notes that often the result is the integration of developing countries' arms industries with those of the big powers. The chapter on Israel confirms the conclusion that greater military production is often accompanied by growing dependence on foreign powers, an interesting paradox. The authors believe that even Israel, possibly the most sophisticated of all third-world producers, cannot achieve substantial independence and must remain dependent more or less permanently upon significant infusion of U.S. technology, money, and even whole weapon systems. It seems that this conclusion holds across the board for all developing nations.

The largest group of weapons self-producers, half of the total analyzed in the book, is to be found today in Asia: the two Chinas, the two Koreas, India and Pakistan, and the Phlippines. Outside of the People's Republic of China, which is significantly more powerful than all other developing countries, India is clearly the largest weapons producer in the Third World. The chapter on India concludes that although India remains dependent on foreign suppliers, her weapon production's effort seems to have strengthened her nonaligned foreign policy and, in general, enabled India to negotiate on a more equal footing with foreign countries.

The three general chapters in the volume offer a framework for further research in this important area. Katz identifies a series of motives for production effort, analyzes some determinants of capability, and discusses briefly the societal implications of arms production, a topic examined in more detail by Bruce Arlinghaus. Neuman's essay examines the arms production phenomenon as part of the larger global arms transfer system.

On the whole, this volume is bound to be regarded as significant by scholars interested in the foreign policy of third-world countries. The approach is empirical and rigorous, and the structure of the volume quite satisfactory. A concluding chapter based on a comparative analysis of the 14 cases could, however, be useful.

ILAN PELEG

Lafayette College

The Soviet State: The Domestic Roots of Soviet Foreign Policy. Edited by Curtis Keeble. (Boulder, Colo.: Westview Press, 1985. Pp. vi + 244. $45.00, cloth; $17.95, paper.)

The "Great October Revolution" of 1917 created a new type of state—the Soviet socialist state—and thereby initiated Soviet foreign policy, which, guided by the principles of foreign relations established by Lenin and reaffirmed at the 26th Party Congress of the Communist Party of the Soviet Union (CPSU), has historically and ideologically remained fundamentally different from the foreign policy objectives of Western democracies. During almost 70 years of clouded relations with the USSR, made all the more ominous by the rise of the USSR to a nuclear superpower and hazy by the passing of Brezhnev in November 1982, the death of Andropov just 15 months later, and the death of Chernenko in

March 1985, the policies of Western governments towards the USSR have revolved around various assessments regarding the nature and objectives of the Soviet state.

According to Curtis Keeble, the former British Ambassador to Moscow (1978-82), in his excellent introduction to this important work:

> It is only too easy to overlook the fact that those who form policy in any country, no matter how great their political power, are constrained to an extent which they themselves may not realize by the political, social and ideological climate in which their own ideas have developed; by the geopolitical situation of their country; by the military and economic resources they can deploy; and by the individuals and institutions through whom they must operate. Dearly though we have paid for past misjudgements, the price of error in relations with the Soviet Union is of another order of magnitude. (p. 2)

With these considerations in mind, a distinguished group of scholars, governmental officials and a journalist gathered in the autumn of 1983, under the auspices of the Royal Institute of International Affairs (Chatham House), "to examine not so much the nature of Soviet foreign policy as the nature of the state and society from which it stems" (p. 2). As such, the objective of the Chatham House meetings, and the objective of the studies contained in this book, has been to look beneath the "external manifestations" of Soviet policies to the domestic "roots" from which they originate (p. 1).

The book itself is composed of Keeble's introduction and 13 comprehensive contributions that fall into three broad categories: the first on history, ideology, and society; the second on the Soviet economy; and the third on policymaking institutions. Individual chapters are by Hugh Seton-Watson ("The Historical Roots"); David Lane ("The Societal Dimension"); Alistair McAuley ("Nation and Nationalism in Central Asia"); John Lawrence ("Religion in the USSR"); David Dyker ("Soviet Industry in its International Context"); Alec Nove ("Soviet Agriculture: Problems and Prospects"); Alan Smith ("International Trade and Resources"); Edwina Moreton ("Comrade Collossus: The Impact of Soviet Military Industry on the Soviet Economy"); Stuart Kirby ("Siberia: Heartland and Framework"); Peter Frank ("The CPSU Local Apparat"); Malcolm Mackintosh ("The Military Role in Soviet Decision-Making"); and Archie Brown ("The Foreign Policy-Making Process"). In the final chapter by Curtis Keeble ("Conclusion: The Implications for Foreign Policy"), a very successful attempt is made to gather together the "threads" of the previous chapters and to "assess the ways in which the nature of the Soviet state may be reflected in its foreign policy . . . to explore the kind of relationship which a Western democracy can reasonably expect to sustain with the Soviet Union" (p. 217).

The Domestic Roots of Soviet Foreign Policy brings together several interesting themes, all of which, in addition to being domestically rooted and of contemporary importance, are presented and analyzed in a scholarly and coordinated fashion. This volume is a welcome addition to the growing body of studies dealing with the motivations and objectives of Soviet foreign policy.

JOHN M. CARFORA

School for International Training (U.S.A.)

The European Parliament: Performance and Prospects. By Emil J. Kirchner. (Brookfield, Vt.: Gower Publishing, 1984. Pp. xiv + 170. $32.95.)

The European Parliament (EP) is either the most ambitious experiment yet in international integration, or it is an expensive opera *buffo* in 434 acts playing in three theaters. The response depends upon one's perceptions, because a good argument can be made for both characterizations.

As an example of international cooperation, consensus-building, and collective democratic decision making, the EP is supposed to provide the basis for the democratic control and oversight of the European Community's institutions and processes. Before 1979, its members were chosen by and from the national parliaments of the EC, but now its members are elected directly by the European population (elections were held in 1979 and 1984). The EP currently has 434 members from all 10 EC members, and its membership will increase with the formal entry of Spain and Portugal into the EC. These 434 representatives do not sit by national groupings. Rather, they sit by the various transnational party groupings, and the specific party grouping is a stronger explanatory variable for their actual voting behavior than is national identity.

The EP is also an opera *buffo*. Seven languages are used, but none are listened to; it has no permanent place to call home—there is the constant shuffle between Strasbourg, Luxembourg, and Brussels. This road show is the result of the EC Council of Ministers' refusal to designate a permanent resting place, and the EP itself does not have the authority to decide where to hold its sessions. The concept of equal pay for equal work is absent: The members' salaries are pegged to those of a member of the respective national parliaments, and thus there is a wide spread (approximately $19,000 for an Irish member of the EP to approximately $48,000 for a French member). The EP resembles more an endless debating club than a legislature with real effective political power. Much of the members' time is spent on nonbinding symbolic resolutions far removed from the EC process (e.g., resolutions bemoaning the condition of the Indo-Chinese boat people and expressions of sympathy for some French and German cows that drowned after extensive flooding).

Both these characterizations are correct, because the EP's presence lies in the future. The elected EP has a political legitimacy that the former appointed Parliament never had, and the potential exists for it to have greater influence in the future. Given these built in limitations to the EP's effectiveness, it is still important to know who its members are, how they behave in parliament, what objectives they have, and how they perceive their role in the overall EC process. Emil J. Kirchner of the University of Essex has provided this information.

Through extensive personal interviewing and written questionnaires, Kirchner provides a fine empirical study of the EP's members. The MEPs are profiled in demographic terms (including social and occupational characteristics); past political experience (both at the national and the European level); how the MEPs spend their time in and out of Parliament; the various issue areas that receive more (or less) attention; an assessment of the EP's impact on the integration process; and a discussion of the MEPs' effectiveness in relation to the future of the EP. The MEPs are well-educated, most have prior political experience, they are competent, and they, at least to this reviewer, address the right issues (most of the time). But the European Parliament has a long way to go before it can exert effective political power.

LEON H. HURWITZ

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The Foreign Policies of Arab States. By Bahgat Korany and Ali E. Hillal Desouki. Boulder, Colo.: Westview Press, 1984. Pp. xii + 354. $35.00, cloth; $15.95, paper.)

This book analyzes the foreign policies of six key Arab countries and the PLO, an organization that has been aspiring to create a Palestinian state for more than 20 years. One is immediately impressed with the organization of this book, its lucidity, depth, and unquestionable value to those who are interested in the Third World in general, and the Arab states in particular. True to its intention, it fills an obvious gap in foreign policy literature. Because the authors adhere to their well-prepared outline, the result is a cohesive, superbly written study.

The authors effectively show that foreign policies neither exist nor function in a vacuum because they always interact with domestic, regional, and global environments. Pivotal to trade and invasion as it has been throughout most of history, the Arab world resembles the cave of winds that echoes not only local, but also international changes and power politics.

Without being bogged down in the "descriptive or prescriptive genre," the authors purposefully present a meaningful and systematic analysis that provides a rationale of the workings of Arab foreign policies. Although the selected political systems share a number of characteristics and norms, each has its own unique capabilities and limitations. Still the Arab world constitutes a collective political culture that is transnational and that injects variables and imposes constraints on foreign policy decisions. Departing from traditional literature that emphasized the psychological, great powers, or model-builder approach, the book charts a new course that skillfully balances the three basic elements of foreign policy: the domestic, regional, and global environments. Decision makers often face the dilemmas of aid versus independence, resources versus objectives, and security versus development. In such tradeoffs, one should not overpersonalize the Third World's foreign policy processes.

The Arab world has its distinctive characteristics based on prevailing opportunities, capabilities, values, and perceptions of roles. Because of its relative cultural cohesiveness, it experiences much interaction, scrutiny, disruption of unity, and vulnerability. In this context, Islam, which has been politically dormant, is emerging as a potent force that may shake the Arab regimes to their roots. Ironically, by the 1980s as a new order—less revolutionary, more stable, and economically oriented—was unfolding, new pressures were being exerted by the forces of Islamic revolution and fundamentalism. In the meantime, each political system of the Arab world has been charting a foreign policy synchronized with its individual environment.

Since independence, Algeria has been trying to balance the conflicting elements of ideology versus pragmatism, revolution versus a business economy, and Algerian, Arab, and Muslim versus

French, European, and Christian. Egypt has always been torn between its domestic needs and regional responsibilities.

As Egypt was beginning to look inward, Iraq was trying to assume Arab leadership. However, Baghdad was frustrated by internal dissension and external pressures, particularly its war with Iran. Libya's dilemma lies in its seeking unreachable objectives. Although it possesses the financial base, it is virtually crippled by its demography, geography, and history.

Oil has transformed the "sandbox" of Saudi Arabia into a formidable financial and political power. Yet Saudi leadership must cope with the problems of modernization and development with the framework of tradition and Islam.

As a revisionist state, Syria is frustrated by its own limitations and external actors. Although recently it was able to gain military power and limited Soviet protection, the serous gap has persisted between its aspirations and reality.

The dominant theme of PLO foreign relations is simply survival. It faces serious problems such as lack of territory, legitimacy, and visibility. Frequently, it has had to choose external support at the expense of its goals. In addition, for the sake of self preservation in its pursuits of a liberation war and political mobilization it has needed to avoid being dominated by the Arab states.

The book contains minor shortcomings that should not diminish its immense contribution. First, data of oil revenues and production could have been updated to 1980 in order to reflect the oil glut and its impact. Second, tables illustrating the predominance of Egypt present a slightly inaccurate picture as countries are freely omitted or added. Third, in the chapter on Iraq, several conclusions are reached without adequate support or substantiation. Fourth, the chapter on Saudi Arabia shows a hazy knowledge of the historical facts accompanying Abdel Aziz's rise to power.

Taking into account the dearth of materials on the subject, this book is certainly a milestone for present and future research on Arab and third-world foreign policies.

MOHAMED EL-BEHAIRY

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Power and Policy in Transition: Essays Presented on the Tenth Anniversary of the National Committee on American Foreign Policy in Honor of Its Founder, Hans J. Morgenthau. Edited by Vojtech Mastny. (Westport, Conn.: Greenwood Press, 1984. Pp. ix + 271. $29.95.)

The purported objectives of this book are three. The first three essays were required to assess how the presence of nuclear weapons has transformed the essence of power. The second group of essays was to consider whether the novelty of nuclear weapons has imposed any limitation on the notion of "realism." The last group was expected to concentrate on challenges to American foreign policy. These tasks are not fulfilled evenly. Only the third set of essays provides readers with an appropriate intellectual challenge. The first two sets of essays are flawed because, in varying degrees, they fail to demonstrate an awareness of some of the pertinent theoretical literature, they do not satisfactorily address the relevant, central questions, and they lack conceptual clarity.

Gary L. Ulmen asserts that the means of collecting information and the techniques and methods of influencing public opinion have become important components of power, and that the Soviet Union is, in these ways, outpacing the United States. Although Ulmen's argument is well presented, it is far from novel. Leaders, particularly those preparing for war, have long understood that "information is power." Moreover, as E. H. Carr made clear nearly 50 years ago, power over opinion is as closely associated with, and is as critical for political purposes, as are both military and economic means. Carr underscored the impact in the twentieth century of technological advances in information/communication systems on the state's ability to influence and manipulate public opinion.

John H. Herz's essay, "Power Politics and Policies of Survival," is flawed in a different way. His argument that power politics in terms of the maximum enhancement of national power cannot solve the issues of national security is well known. The logic of the argument, however, is founded on the questionable assumption that national interest is an easily identifiable concept that can serve as a guide to foreign policy formulation. But as several studies have demonstrated quite convincingly, national interest lends itself better to the process of legitimizing policies than functioning as a precise criterion to help policy makers determine what actions to implement.

The essays in the second section of the book are well written and quite informative, particularly if one wishes to learn about the foreign policy views of the Green party, New Left intellectuals, or The Institute for Policy Studies. These groups, the authors remind us, question both the intellectual utility and moral soundness of a continued adherence to a "realist" perspective of the world. But the essays never attempt to address the issue of whether the United States is experiencing a retreat from realism.

The last group of authors by and large succeeds in fulfilling its assigned task. Dankwart A. Rustow argues quite cogently that the most important challenge faced by the United States in the Middle East is not to insure the continued supply of oil, but to preserve the diversity of the Middle Eastern state system. Stephanie Neuman is very convincing in her contention that factors of scale shape the world's arms production and transfer system into a hierarchy of capabilities, with the United States well entrenched at the top. In a well-balanced analysis of Jeane Kirkpatrick's tenure as U.S. ambassador to the United Nations, Seymour Maxwell Finger concludes that the interests of the United States are not likely to be well served by strategy of confrontation. Finally, following an appraisal of the expectations of American leaders in the early stages of the framing of détente and the present state of relationship between the two superpowers, David Sidorsky notes that any attempt by the United States to restore the policies of détente must be preceded by a careful assessment of the severe dilemmas that would plague their implementation. Indeed, he questions the wisdom of even attempting to surmount these obstacles and suggests that détente not be restored.

In sum, this book falls short of its intended objectives. It is a malady quite common among edited books: It lacks direction. Such books should not merely allow authors to communicate their individual findings and views. They must have a governing core idea, encompass a set of clearly defined objectives, and address them systematically.

ALEX HYBEL

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Asian Perspectives on International Security. Edited by Donald Hugh McMillen. (New York: St. Martin's Press, 1984. Pp. xxii + 226. $29.95.)

This book is a collection of papers given at a conference at the Strategic and Defense Studies Center at The Australian National University. The largest number of contributors teach in Australia, but the majority work in a variety of other Asian countries and one in the United States. The book was planned to contribute to a better understanding of "Asian perspectives of the world, and in particular of the crucial issues related to contemporary international security." The book succeeds well in presenting the individual perspectives of a variety of Asian countries, from the Middle East and the Persian Gulf, to South Asia, the Soviet Union and East Asia.

The general tenor of the book is well summed up in the quote from Hans Morgenthau with which the editor begins the Preface to the book. Morgenthau wrote that in international politics, the claim to universality that inspires the moral code of one group is incompatible with the identical claim of another group. Because the world has room for only one, the other must yield or be destroyed. The readings provide plenty of evidence that

> carrying their idols before them, the nationalistic masses of our time meet in the international arena, each group convinced that it executes the mandate of history, that it does for humanity what it seems to do for itself, that it fulfills a sacred mission ordained by Providence, however defined. (p. xiii)

The Soviets invade Afghanistan, the Vietnamese invade Kampuchea and Laos. (Carlyle A. Thayer skillfully seeks to explain the motivation, decision-making process, and rationale for Vietnam's attempt to protect itself from its larger neighbors and to consolidate its own hold on the rest of former Indochina.) Iran and Iraq, Israel and the Arabs wage vicious battles colored by a sense of destiny, an exaggerated sense of their own virtue, and religious and quasireligious symbolism. On the other hand, the readings also provide evidence that foreign policy is sometimes more a product of elites than masses, that nations occasionally exercise considerable restraint in their foreign policy, are driven by defensive rather than offensive motives, and are ready to grasp at opportunities for accommodation rather than conflict when the opportunity arises.

In a chapter on the Soviet Union's security outlook, Paul Dibbs argues that whatever the outsider's interpretation, Soviet actions appear to the Soviets as largely defensive in nature and necessary if they are to continue to exist in an unfavorable environment. In a similar vein, Zalmay Khalilzad argues that the Soviet action in Afghanistan represents a continuation of Moscow's low-risk pattern of foreign policy because it was in the Soviet interest to intervene, and there was likely to be little opposition from outside. The foreign policies of ASEAN members are generally defensive in nature and more concerned with finding solutions to problems, especially Kampuchea, than in creating mischief. To take another example of restraint, Fuji Kamiya shows Japan's foreign policy is anything but militaristic.

The chapters that are most useful and best add to what is already known about these countries are those that use a comparative approach. Robert Springborg has a very useful chapter on the mirror image types of behavior that characterize both the Israelis and the Arabs in the Middle East. In a chapter on the Persian Gulf, Mohammed Ayoob compares the perspectives, motivations, and behavior of Iran, Iraq, and Saudi Arabia. In these essays on Asian perspectives, there is no shortage of "superpower bashing," but it is interesting that the U.S. and the USSR rate only third on a threat perception list for Iran, after internal-ideological and Iraq, and neither appears on a list of four threats for Iraq. For Saudi Arabia, both Iran and Israel outrank the Soviet Union. In the end, it is still the regional enemy that is the most threatening.

Some Asian commentators, far from fearing superpower intervention, would like to see a stronger U.S. stance. Sang-woo Rhee argues in a chapter on South Korea that the United States underestimates the threat from the North, and Pervaiz Iqbal Cheema believes that détente helped the Americans to disengage from Asia but undermined the security of the South Asians, especially Pakistan, which was once closer to the Americans. It may be indicative of the diminished role of the United States in Asia that no chapter on U.S. policies is included.

This book draws together a diverse group of essays on countries scattered over a broad geographical area and several international subsystems. The book is not well integrated nor is it developed around any well-defined theme. In some ways this is its strength, however, because many of the chapters contain new material and insights that help to explain the policies and outlooks of individual countries in a fresh perspective.

WAYNE BERT

Reston, Virginia

United Nations Law Making: Cultural and Ideological Relativism and International Law Making for an Era of Transition. By Edward McWhinney. (New York: Holmes & Meier Publishers, Paris: UNESCO, 1984. Pp. xi + 274. $34.50, cloth; $19.50, paper.)

This book is the third in UNESCO's new series, "New Challenges to International Law," that was inaugurated in 1979. McWhinney has undertaken the task of analyzing the processes, arenas, and actors involved in law making within the United Nations. As the subtitle suggests, we now live in an era of transition, having departed from the bipolarity and twin political-military blocs dominated by the United States and the Soviet Union, into an interim of a polypolar world community with a plurality of significant players. Throughout the work, McWhinney discusses relativism concerning classical rules of international law and the normative quality of these rules as tested by reason today.

McWhinney examines the traditional sources of international law and then illustrates contemporary trends with an emphasis drawn from the sources of equity and *jus cogens,* which represent the new legal-realist conception of international law. He considers the roles and contributions in law making in and by the General Assembly, the Security Council, the International Law Commission, and the Secretary-General, suggesting how gaps in the United Nations Charter could have been filled through innovation. His approach is not polemic but rather a review of premises supported by precedents in various problem solving situations. He is disappointed with the lack of productivity and apparent ineptness of the International Law Commission, but cogently accounts for its shortcomings. With respect to nation-disputants' resort to the International Court of Justice, McWhinney provides an ample discussion of past and current attitudes towards international adjudication, the watershed decision of the Court in its 1966 South-West Africa case, Second Phase, a review of cases showing judicial activism, and the ledger concerning the 1972 institutional reforms within the Court.

Not unexpectedly, McWhinney dwells on the needed reforms to the Charter of the United Nations, specifically highlighting the Special Committee's 1981 report and 1982 supplemental report and what was sought to be achieved by the 1982 Manila Declaration on the Peaceful Settlement of International Disputes. The roles, shifting balance, and interplay of the United Nations' professional bureaucracy are explored. Herein we find emphasis given to the work of the Secretary-General, General Legal Counsel, the Sixth (Legal) Committee, andd the International Law Commission.

Finally, McWhinney reacts to ideological pluralism as it accounts for contemporary international law values in relation to a number of pressing problem areas: self-determination of peoples, the new international economic order, the use of force, disarmament, the common heritage of mankind, and the world information and communication order.

Although this book is scholarly, inquiring, and represents an excellent appraisal of the gains in legal development in the United Nations for the world community, it is at times ponderous and not easily readable.

DANIEL C. TURACK

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The USSR in Third World Conflicts: Soviet Arms and Diplomacy in Local Wars, 1945-1980. By Bruce D. Porter. (New York: Cambridge University Press, 1984. Pp. viii + 248. $29.95.)

Within the last two decades Soviet military involvement in third-world conflicts has become an increasingly salient aspect of international politics, redrawing the pattern of international relations. Concern over Soviet "expansionism" helped undermine the rationale of détente, and successive demonstrations of Soviet capability for military intervention in the Third World continue to be invoked on behalf of increased military expenditures by the United States. Security analysts have suggested the escalation of a third-world conflict as the most plausible scenario of the outbreak of World War III.

Porter's book begins with a historical survey of Soviet involvement in third-world conflicts, marking a series of "firsts," from the first overt arms agreement (with Egypt in 1955) through the first confirmed combat participation by Soviet fighter pilots (in the Yemeni civil war), to the first Soviet invasion of a third-world country—Afghanistan. In a chapter assessing Soviet military capability for intervention, Porter traces the increments in the magnitude and sophistication of military aid as well as in the logistical capabilities to deliver the aid with speed and efficiency. However, Porter's chief contribution to the consideration of Soviet military involvement lies in his approach: The core of the book is a comparison of five cases on the basis of a comprehensive configuration of interrelated factors. Although Porter's conclusions vary in significance, the analysis lends them the authority that is often lacking in less systematic, less comprehensive approaches to the subject.

The cases were selected according to the following criteria: The contenders were noncommunist, were involved in conventional warfare, Soviet arms shipments and other aid were delivered during the hostilities, and this military aid played a significant role in determining the outcome of the conflict. On this basis Porter winnowed five out of the two dozen or so instances of Soviet military intervention: the Yemeni and Nigerian civil wars (1962 to 1969 and 1967 to 1970, respectively), the October Arab-Israeli war (1973), the Angolan civil war (1975 to 1976), and the Ogaden war (1977 to 1978). Each case study examines the nature of the local dispute, the diplomatic relationship between the Soviet Union and the "client," the type and logistics of weapons shipments and other military aid, Soviet-American interaction, the "China factor" (yielding least fruitful conclusions), and the outcome of the conflict.

Porter's conclusions along these dimensions range from important conceptualizations and reexaminations of controversial aspects of the subject to the familiar and even questionable propositions. For instance, Porter attaches considerable significance to the fact that in each case the Soviets seemed to have chosen the winning side to back and ascribes the choice mainly to the effectiveness of their research and intelligence-gathering capabilities. This seems to be an oversimplification not only because of the criteria used to select the cases (only those in which Soviet intervention was critical in determining the outcome, thereby making the matter of choosing the right side of secondary importance—they could *make* a winning side rather than choosing it), but also because it overlooks the relative lack of choice in at least some instances. In the Ogaden war the choice was made for the Soviets when they failed to dissuade Somalia from fighting over the Ogaden. A similar argument could be made in the case of the October war.

Some of Porter's conclusions contribute to the debate over the nature of Soviet involvement in the Third World. Porter's argument that "the Soviet Union cannot plausibly be accused of having instigated or caused the local conflicts in which it became involved" (p. 219) undermines the perception of Moscow as actively seeking opportunities for military intervention in third-world conflicts. Similarly, Porter's conclusion that the Soviets "seem to be acutely sensitive to the risks involved in any conflict that might threaten vital U.S. interests" (p. 231) adds weight to the argument that the Soviets have been careful to avoid any direct confrontation with the United States. On the other hand, his suggestion that the Soviet invasion of Afghanistan "somewhat reversed the trend toward diplomatic accommodation on the part of the Third World" with the Soviets is weakened by his misinterpretation of the result of the Seventh Non-Aligned Summit Conference (which he erroneously dates as February, 1981—it was held in Delhi in March, 1983) as a "diplomatic setback suffered by the USSR over the invasion" (p. 240).

Most notable of Porter's conclusions is his conceptualization of the impact of Soviet intervention in third-world conflicts: "By advancing incrementally and by carefully choosing the places and times of its involvement. . ., the Soviet Union has substantially increased its latitude of action in the world. The cumulative effect has been an evolutionary erosion of the post-war order, a gradual change in the nature of the [rules] of the game. . . . What the USSR achieved in the Third World between 1973 and 1980 would have been totally unacceptable to the United States only a few years earlier; two decades earlier it might have led to war" (p. 242).

RITA P. PETERS

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International Nuclear Technology Transfer: Dilemmas of Dissemination and Control. By Benjamin N. Schiff. (Totowa, N.J.: Rowman & Allanheld, 1983. Pp. xiv + 226. $35.95, paper.)

This is an excellent, in-depth study of the history of the politics of the International Atomic Energy Agency (IAEA) created in 1957. IAEA is the international organization whose primary task is to insure that peaceful nuclear technology is not diverted to military purposes. It does so through two major programs: a technical assistance program and a safeguards program. Schiff develops a number of arguments. First, politically the conflict has shifted from West and East to a confrontation between developed and developing states. Second, as in most cases where technology is involved in international relations, the developing states seek a redistribution of power and access to nuclear technology while the developed states are primarily concerned with limiting the spread and controlling access to nuclear technology. Third, the level of political conflicts generated by nuclear technology is leading to heightened conflicts rather than greater cooperation as manifested in the politics of IAEA and implementation of its two key programs.

Fourth, Schiff holds that the politics of the developed and developing groups of states have led to a paradoxical situation that insures that the regime works at crosspurposes. The developed countries have "created incentives for developing countries to pursue autarchic development" leading to a "unit veto Gotterdammerüng system" (pp. 218, 17) that would reduce any incentive for negotiation. The developing states by their "obstructionism in international forums and reluctance to accede to the expansion of safeguards activities" have encouraged hegemonic manipulation of international regulatory institutions and unilateral restraints on technology transfers by developed states (p. 218). Fifth, the author provides a creative application of "regime theory" to the nuclear nonproliferation regime made up of a web of treaty commitments, tacit agreements, an international organization. The regime's key principles and rules are identified and its historical evolution is analyzed, going through the phase of effective controls, through the phase of increased multilateral cooperation and the reversion in direction after 1973.

The book is divided in five chapters. Chapter 1 considers the interaction between nuclear technology and regime politics, setting out the regime's main lines. Chapter 2 offers an excellent history of control efforts, specifying the central role of IAEA. Chapters 3 and 4 provide an original, detailed description of the safeguards and technical assistance programs not usually considered. Finally, chapter 5 concludes by developing the theme that "international cooperation is more likely to decline than advance" in the years ahead (p. 3).

The book's major contribution is its detailed analysis of the regulatory problem of an international technical organization and its programs. Schiff has been able to rely in his research on the extensive archives of the IAEA and informal documents of its governing board as well as numerous interviews with key participants. This kind of access is reflected in the detailed descriptions that offer a unique perspective on the inner workings of this key international technical organization.

A number of weaknesses bear noting. Schiff considers that certain European states would favor the spread of nuclear technology "without regulations" (p. 24). This is very probably a misguided and too glibly stated viewpoint. Second, one wishes that the Carter and Reagan administrations' efforts had been considered more fully in the context of IAEA's work. Third, the book generally downplays the importance in recent years of the decentralizing tend in world nuclear commerce and the resulting growth of new enrichment and reactor production facilities with overcapacities. Fourth, perhaps the conclusions are somewhat more pessimistic than the evidence warrants. No state, after all, has begun a nuclear-weapon program since the Non-Proliferation Treaty came into force in 1970. Undoubtedly, this text is an important contribution and a very useful one considering the upcoming five-year NTP review conference. It would be the ideal companion to Robert Boardman and James Keeley's *Nuclear Exports and World Politics* (St. Martin's Press, 1983).

JOHN R. MCINTYRE

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American Foreign Policy in a Hostile World. By Simon Serfaty. (New York: Praeger Publishers, 1984. Pp. 398. $39.95.)

Much more than our colleagues who focus on domestic policy matters, students of American foreign policy seem drawn to the historical aspects of the subject. It's not entirely clear why this is so, but the result is a welcome gain in depth and perspective for much foreign policy scholarship. So widespread is this proclivity within the field that it has given rise to a distinct scholarly genre: the analytic and narrative survey of American foreign policy since World War II. Not quite textbooks in the ordinary sense of the term (although frequently useful for pedagogical purposes), books of this type have typically sought to advance their authors' own interpretation of the patterns and performance of American diplomacy in the postwar world. Some—most notably John Spanier's *American Foreign Policy since World War II* (Praeger Publishers, 1960) and Seyom Brown's *The Faces of Power* (Columbia University Press, 1968)—have proven quite popular and influential within the field.

Simon Serfaty's latest book offers a useful addition to this genre. Writing from a perspective of prudent realism, Serfaty develops a thoughtful, well-supported account of the principal foreign policy decisions and events of the past 40 years. Copious citations to the literature undergird the work without deflecting the author from his own distinct point of view. The theme throughout the volume is the necessity for sober, pragmatic policies that will secure the nation's abiding interests amid a welter of external threats. Against this determinedly nonromantic standard, Serfaty finds the nation's postwar record to be, at best, unconstant. Repeatedly he warns against excessively ambitious policies attuned more to domestic values than global realities. He particularly criticizes "the way in which American foreign policy is presented to the public, wrapped in a sweeping rhetoric that commits the country to grandiose but often unrealistic objectives" (p. 3).

Part 1 covers much of the same ground as Serfaty's 1972 work, *The Elusive Enemy* (Little, Brown). By far the longest section of the book, accounting for six of nine chapters, it spans the years from the beginnings of the cold war through the Vietnam debacle. Nearly any reader, from undergraduate seeking a lucid introduction to seasoned scholar wanting an interesting review, will benefit from Serfaty's clear narrative and analysis of the period. In addition to the expected coverage of political and strategic matters, Serfaty does an unusually good job of treating economic issues. Throughout Part 1 Serfaty is most effective in recounting the international setting confronting American policymakers. His treatment of the domestic context is somewhat less complete. In one instance where he does devote substantial attention to domestic policy sources—the role of John Foster Dulles during the Eisenhower years—his analysis is not altogether persuasive. Presenting Dulles in the familiar garb as "a preacher in the world of politics" animated by "a single-minded perception of international communism as the enemy" (pp. 135, 137), Serfaty's account suggests that Dulles was the prime mover behind foreign policy decisions during these most crucial years. Ignored altogether are the findings of recent scholarship including, most notably, Robert Divine's *Eisenhower and the Cold War* (Oxford University Press, 1981), which establish Eisenhower himself as his administration's principal policy architect, relegating Dulles to the highly visible but less significant role of policy spokesman and executor.

Part 2, "America in a Hostile World," consists of three good chapters treating the principal foreign policy thrusts of the post-Vietnam years. Chapter 7 offers up an insightful critique of the much-dissected Nixon-Kissinger strategy. Chapter 8 deals with the frustrations of allied relationships in the new age of multi-issue agendas. Chapter 9 treats the disappointments of the Carter years and the ideological outlook of the Reagan administration. A fuller account of the Reagan approach would have been welcome, but Serfaty's brief survey well captures the administration's essential mindset.

On balance, Serfaty has contributed a solid addition to the literature on postwar American foreign policy. Thoughtfully constructed and well written, the book deserves a wide readership.

EUGENE BROWN

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The Political Economy of African Foreign Policy: Comparative Analysis. Edited by Timothy M. Shaw and Olajide Aluko. (New York: St. Martin's Press, 1984. Pp. xiii + 397. $37.50.)

Timothy Shaw and Olajide Aluko view their edited volume as part of a "second wave" (p. 2) of scholarship regarding African foreign policy that avoids the purely descriptive analysis of crises, personalism, and other idiosyncratic factors so characteristic of the traditional literature. Instead, their chosen political economy approach emphasizes the interaction of systemic substructures of international capitalism with the emerging class configurations in each of a selected cross section of African states. Shaw argues that this materialist focus "represents a considerable advance over the simplistic and uncritical forms of diplomatic history and ideological investigation characteristic of the 1960s" (p. 377).

Does their method fulfill its advance billing and "go beyond superficial and orthodox investigations towards a more fundamental and radical explanation" (p. x)? Certainly the major strength of the book lies in its emphasis upon underlying processes and elements of continuity at work in seemingly inchoate and unpredictable African environments. Nevertheless, the editors' lack of attention to both bureaucratic structures and characteristics of personnel who translate the dominant class interests into foreign policy detracts from their otherwise commendable effort. That class interests are quite straightforwardly perceived by African elites and then accurately translated into policies appropriate to fulfilling these objectives seems a somewhat dubious assumption especially in such stressful environments. What also seems questionable is the tendency of these political economists to assume that an underlying rationality can and must explain observed phenomena. Some greater attention is required to the possibility that political outcomes frequently result from a patchwork of the unintended consequences of "satisficing behavior" designed only to muddle through difficult situations. Similarly, there are pitfalls in too severely downplaying the impact of individuals in relatively new states where customary and habitual guidelines as well as rational-legal norms are not firmly established.

Do the case studies reflect the range, diversity, and relative significance of African foreign policy problems and outcomes? Shaw and Aluko have done an excellent job in choosing from the large array of sub-Saharan states cases representative of differing size, economic capacity, colonial background, type of political economy, and intrinsic importance to the African scene. Although the result may be slightly Anglocentric, no one can seriously question the inclusion of such pivotal states as Ghana, Kenya, Nigeria, Tanzania, Zambia, Zimbabwe, Botswana, and South Africa within the 13 selected.

How effectively do the case studies utilize the method of political economy and demonstrate its utility as an analytic construct? As in many collections of essays, there is considerable variation. Some, like the chapters on South Africa, Tanzania, and Zimbabwe, make rather sporadic use of the concepts of class and substructure developed by the editors. They do present informative and insightful surveys of foreign policy issues and options in those countries, but seem more orthodox than part of a "second wave." Others, including the chapters on Botswana, Ghana, the Ivory Coast, and Nigeria follow the jargon of political economy too narrowly, leading to the emergence of oversimplified and somewhat stereotyped imagery that neglects the nuances and specificities of the particular setting. For example, identification of Seretse Khama as simply "the hereditary leader of Botswana's largest tribe" (p. 63) misses the complex synthesis of traditional, entrepreneurial, interracial, and international elements that he epitomized.

The most outstanding essay in this volume is Michael G. Schatzberg's multidimensional assessment of Zaire's foreign policy. Organizing his chapter around different dimensions of dependency, Schatzberg uses well-chosen examples to illustrate how general themes in the Zairian political economy impact on specific foreign policy situations. He demonstrates how "diversification of dependency" (p. 295) permits Mobutu to utilize his considerable political skills, but results from a structured environment transcending the particular individual. Richard Higgott's chapter on Niger presents an especially stimulating analysis of how the development of uranium resources permitted the accentuation of existing trends toward a multilateral dependency situated in that country's landlocked geopolitical realities. Christopher Clapham's analysis of Ethiopia is noteworthy for its attention to the present asymmetry between economic and military dependency.

Overall, the book presents some graphic insights on the coexistence of increasing economic marginalization with processes of political maturation reflected in African foreign policies (p. 373). It also calls attention to changes likely to affect future transactions on the continent. As a minority of African states play a semiperipheral role due to a more fortuitous situation in the global political economy, their relationships with the majority that remain impoverished and peripheral will grow more problematic.

RICHARD WEISFELDER

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Quantitative Indicators in World Politics: Timely Assurance and Early Warning. Edited by J. David Singer and Richard J. Stoll. (New York: Praeger Publishers, 1984. Pp. xiv + 221. $22.95.)

Singer and Stoll's stated purpose is "to exploit what might be called the 'spin-off' from a number of basic research projects, in order to move ahead on the policy relevant front while waiting for major advances toward more coherent, data-based theory to emerge in the world politics field" (p. 2). Their volume represents partial success at that endeavor.

Part of the reason for having achieved only partial success on these counts lies in their view of policy relevance. The editors state in their introduction that "at this time, in choosing between manipulability and predictive accuracy, we believe the latter merits the higher priority" (p. 4). I find myself a bit bewildered by this comparison, because it is at the research design stage, before the scholar has answers about predictive accuracy, that one typically chooses whether or not to include policy manipulable variables.

The first of the book's four sections deals with predicting subnational conflict. Richard C. Eichenberg, Brigitta Widmaier, and Ulrich Widmaier report on a Science Center Berlin project carried out from 1977 to 1979. Relying heavily on *World Handbook* data, the authors estimated cross-section models of internal war and collective protest using such variables as energy consumption and population. After analyzing such data for 39 countries and undertaking statistical projections based upon their cross-sectional (non-time series) data, they are appropriately cautious in interpreting their results.

Daniel Geller writes the other essay in this first section. Geller presents a set of structural equations that represent his attempt to develop a multistage sociopolitical model of civil strife. The model is thoughtfully reasoned, and Geller undertakes a fairly stringent test by attempting to forecast conflict for 20 Latin American nations. The mixed results seem to be partially attributable to model imperfections, but more likely due to an independent variable data base that is limited by data availability.

Part 2 of the book is titled "Predicting Foreign Events and Foreign Involvements." Its two component chapters are written by Zeev Maoz and by Richard Stoll. Maoz presents one of the chapters with the highest potential for practical relevance to policymaking. He employs an experimental design to examine the utility of using Bayesian analysis to improve intelligence forecasts. Evidence presented points to advantages of Bayesian analysis for both surprise attacks and false alarms. Although aspects of the research design call those results into question, Maoz acknowledges many potential criticisms and addresses them.

Stoll develops an admittedly simple model to account for past U.S. behavior in cases where he categorizes military force as having been used as a political instrument. He then uses probit analysis to produce conditional forecasts of U.S. actions. His hope is to create a prototypic early warning indicator system. I assume he would join my hope that such systems would be informed by increasingly more sophisticated theory.

Chapters by Frank Whelon Wayman and I. William Zartman comprise Part 3 of the volume, "Predicting Serious Disputes and Conflict Resolution." Wayman utilizes judgments of State Department and academic specialists in an attempt to predict armed Middle East conflict. WEIS and COW data are employed to measure the conflict. Disparity is found between the experts' hostility/friendship judgments and the picture drawn from the event data. One must either conclude that expert judgment is flawed or that not every dimension of friendship and hostility is captured by the data. This second interpretation can be seen as finding some support in Zartman's brief methodological note chapter. In it he concludes that for many research questions involving conflict, diplomatic analysis may well provide better answers than presently available aggregate data.

Charles W. Kegley, Jr. and Gregory A. Raymond's chapter, "Alliance Norms and the Management of Interstate Disputes," serves as the book's conclusion. Kegley and Raymond set out to investigate whether changes in the reliability of alliance commitments have an impact on the pattern of international conflict. They are to be commended for having asked an important question, for acknowledging that a central assumption of their study is the ability to accurately measure the strength at any particular time of norms regarding binding obligation, and for devoting a number of pages specifically to the issue of policy implications. Alliance commitments are measured by content analysis of 244 authoritative legal treaties written since the Congress of Vienna, while COW data serves as the basis for measuring major power disputes. They present a set of multiple regression equations relating structural flexibility and treaty commitments to conflict frequency, scope, and intensity. In their conclusion, the authors (while recognizing a "macro-micro" disparity) draw implications for policymakers debating whether to adhere to treaty commitments. That is an important debate to focus upon, whether or not one agrees with their assumption regarding measurability of an at least partially *perceptual* issue such as strength of alliance norms at various points in history.

Singer and Stoll have presented seven studies that capture the mainstream of quantitative international politics. Whether these generally well-written essays yet offer "policy relevant spin-off" is not entirely clear.

DONALD A. SYLVAN

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Paths to International Political Economy. Edited by Susan Strange. (Winchester, Mass.: George Allen & Unwin, 1984. Pp. ix + 209. $25.00, cloth; $10.50, paper.)

"Political economy" means many things to virtually as many people. Prefixing the term with "international" has restricted its meaning somewhat, but the problem of definition remains. The classical writers used the term loosely to imply an open-ended study of the relationships of politics and economics using eclectic, rather poorly developed, tools. But the past 200 years have witnessed what Strange appropriately calls (p. ix) an academic "enclosure movement," fencing off politics to political science and economics to economists.

One of the confusions today derives from the failure to distinguish between subject matter to be studied and the methods and theories used to study it. At the extremes, there are the "economics of politics" in which economists apply economic theory to political subjects such as voting and coalitions, and the "political science of the economy," in which political scientists (rarely) try to apply political theory to the economy. Economic theories of politics abound, although there are very few truly "political" theories of economic matters, however. Political scientists have not been very interested in a political theory of economics despite repeated calls by the likes of Robert Dahl and Charles Lindblom over the past 25 years.

In between these extremes there are all kinds of mixtures of subject matter and methods. A fair amount of this is collaborative work of economists and political scientists, barely cemented together and hardly very "interdisciplinary." And if it is not well-integrated, it matters little if the cementing is done within one person's head instead of two.

Much of the currently fashionable international political economy consists of the study of inter-*governmental* relations, the content of which happens to be economic. Given the state-centric view of politics in both political science and international relations, this is all too often the defining characteristic of the field.

This book moves beyond this limited definition but only so far, and it illustrates the difficulties in so doing. It assumes that there is a subject matter called international political economy and that there are various disciplinary paths to examine it —through economic history, demography, international legal analysis, etc.—in addition to economics that is not represented (although Strange has good things to say about development economics). The basic idea is to provide a short guide to some of the basic concepts and literature of the subfield.

Paths to International Political Economy could be used as a text, and Roger Tooze's opening chapter provides a solid introduction to both the field and its various perspectives. He, too, notes the contentious nature of the field's content. The review of approaches is sound, but I question the epistemological rigidity implied on page 7: "what you ask and how you go about getting the answer nearly always determine that answer for you."

David Wightman's short, convincing brief for economic history rightly criticizes the ignorance of history and the academic fads that characterize much contemporary, especially American, thinking about world politics. There is not much new under the sun, and we won't be able to recognize what *is* new if we don't have our history straight. Moreover, policymakers, like generals preparing for the last war, spend too much time looking over their shoulders and misusing historical analogies like "No More Munichs" or "No More Vietnams."

Nicholas Demeraths's review of demographic factors seems competent enough but supplies little surprising or new even for a nonspecialist. Likewise, the chapters on the ecological approach and technology would probably be of use to students but will not offer much to the professional. Both are creditable discussions, however. Part of the problem, especially in the essay on technology by Roger Williams, is simply the amount of material to be covered in a short space.

Easily the best chapter is the essay by David Calleo and Strange. They effectively berate the weaknesses in conventional economic analysis of money matters. Political scientists (and students) will appreciate their emphasis on demystification of technical monetary concepts and issues. Especially impressive is their incisive analysis of the hypocrisy in American liberal and conservative, academic and policymaking, economic analyses of inflation and monetary management (pp. 107-116). They forlornly conclude that the bulk of research in the monetary area has been a "technical obligato to the themes set by shortsighted government policies" (p. 117).

Another fine contribution is the article on the politics of trade by L. Rangarajan, which covers a great deal of ground: who trades with whom, the postwar trading system, the rules of the game, and the plethora of trade conflicts among different nations. Particularly interesting is his emphasis on dynamic change in the system and the relative contrast between the North-GATT-free-trade links and rules and much less favorable South-ICA-protectionism rules and links (pp. 149-152).

Sal Picciotto's overview of the changing roles of international law is appropriately skeptical of the neo-Grotian view that complex interdependence might be a foundation for greater international legality. Instead, he cogently argues that "the dominant trend seems to be . . . towards forms of international state corporatism, in which disputes are settled and issues managed by direct contacts among corporate and state managers" (p. 180).

Last, Strange's fine concluding chapter argues that we (especially students) should not discount international relations at least as a point of departure for understanding international political economy. It contains commonplace insights that are often overlooked, for example, security is the prime value and power of various types is central.

Although this book serves its purpose as a pocket guidebook well enough, in some ways it is disappointing. The essays range from first-rate to just passable, and with some notable exceptions there is little here that the specialist does not already know. Perhaps more importantly, one has the uneasy feeling that although some of the literature being published is exciting, the methodological and theoretical advances are simply not commeasurate with the increasing significance of the events and topics that it seeks to understand.

PHILIP L. SHEPHERD

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Nonstate Actors in International Politics: From Transregional to Substate Organizations. By Phillip Taylor. (Boulder, Colo.: Westview Press, 1984. Pp. xvii + 247. $30.00, cloth; $13.95, paper.)

To a considerable degree Taylor achieves his stated purpose of providing "a basic text on nonstate actors that exist outside the United Nations and its associated international bodies. Hence, the content differs from the many texts on 'international organization' that focus almost exclusively on the United Nations and contain only one or perhaps two chapters on 'regionalism' or the European Community alone" (pp. xv-xvi).

The problem with this book, however, is how Taylor achieves his stated purpose and the points at which he deviates from it; that is, where he seeks to go beyond his modest statement of purpose.

The book begins with a fairly traditional discussion of the limits to a state-centric approach to world politics. Contained therein, however, is this fairly bold assertion: "it is crucial to note that most of the incidents of international conflict since 1945 have involved largely, or exclusively, nonstate actors" (p. 5). Of course, he is technically correct; but it seems a bit inappropriate to base part of one's argument against a state-centric approach to world politics on the role of nonstate actors in such conflicts as Korea, Vietnam, Iraq-Iran, Angola, or Afghanistan. More importantly, perhaps, it seems a bit misplaced to have a discussion of the relative merits of a state-centric approach to world politics in a book that intentionally omits some of the most important nonstate actors and fails to compare them systematically with the power of states (as could have been done, for example, in Taylor's discussion of multinational corporations, wherein he slights recent statist discussions). Likewise, in discussing the relative impact of nonstate actors on world politics, Taylor surprisingly understates such organizations' "latent tasks," so insightfully identified a decade ago by Krause and Nye.

The introductory materials of the book also include a fairly traditional discussion of theories of international integration, in which Phillippe Schmitter's classic analysis is highlighted. This discussion provides one of the leitmotifs for the rest of the book, which is a bit surprising given that Taylor recognizes that such theories offer little insight about security organizations, much less those of the Eastern bloc, nor about multinational corporations or labor unions. Indeed it's surprising how much space Taylor devotes to assessing the degrees of integration of various nonstate actors and integration theory itself while he holds the view that: "The theories of MNCs are generally no more successful in explaining the behavior of nonstate actors in world politics than are theories of international political integration. Both theoretical approaches suffer due to ideological biases, insufficient testing efforts, and very little consensus among theorists" (p. 212).

Not surprisingly, then, the bulk of Taylor's book is atheoretical. It provides a fairly comprehensive, although necessarily superficial discussion of a wide variety of nonstate actors, including economic, political, and cultural/ideological international governmental organizations; security alliances; multinational corporations; international labor unions; and transnational ethnic groups. For the larger of such organizations he presents the organization's history and development, institutions, processes of decision making, as well as assessment of their state of integration. Much of this material is like that contained in *EUROPA Yearbook* and *The Political Handbook of the World.* But Taylor's presentation, especially in the paperbound format of the book, is readily available to students. Moreover, he has usefully included discussions of defunct organizations (like SEATO and the East African Community), but unfortunately these are very cryptic.

Also disappointing is the lack of up-to-date information about many of the organizations discussed and the insubstantial discussions of many organizations' most controversial and important activities. For example, there is no discussion of the cases heard by the European Court of Justice or the controversies surrounding the OECD's Code of Conduct. Even in terms of the structural evidence more abundantly presented throughout, vital information is sometimes omitted. For example, the discussion of the European Court of Human Rights almost exclusively focuses attention on cases brought to final resolution rather than making clear that the Court's influence is considerable in terms of cases resolved out of court.

Thus, altough Taylor's book makes no real theoretical contribution to the literature, even in terms of systematically testing a non-state-centric model of world politics or integration theory, and is less interesting than it might have been, it still has a place in many undergraduate classrooms as a supplementary work on international organization broadly understood. Hopefully, in a revised edition, Taylor will use his space a bit more effectively by bolstering the discussion of the dynamics of nonstate actors and downplaying, if not actually eliminating, his discussion of integration.

MICHAEL G. SCHECHTER

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Governance in the Western Hemisphere. Edited by Viron P. Vaky. (New York: Praeger Publishers, 1983. Pp. xxiv + 532. $39.95.)

This is an important and comprehensive book on international political and economic relationships in the Americas. It is the outcome of an Aspen Institute project financed by the Organization of American States and several multinational business and trade organizations. The book consists of 19 commissioned background papers by distinguished Latin Americanists preceded by a report of the Western Hemisphere Project Steering Committee. The Steering Committee, a kind of Hemispheric Trilateral Commission, consists of 22 eminent U.S. and Latin American diplomats, officials of international organizations, business executives, and other notables who have culled ideas from the background papers and present them as recommendations with their imprimatur.

The report explains that "most of the history of inter-American relations . . . can be written in terms of the process by which the United States sought to legitimize its use of power and Latin Americans sought to cope with it" (p. 7). Such a history creates problems for a book of this kind, which is intended to benefit the "hemispheric community" generally. The authors' solution is to adopt a hemispheric perspective (including Canada), a kind of "news from nowhere" approach that assumes a collective interest in peaceful dispute settlement and social/economic development. This is internationalist, reformist, rational-actor social science with a top-down approach geared to influencing current decision makers. These virtues preclude others, however. Socialist perspectives are notably absent as are criticisms of multinationals, the World Bank, or the International Monetary Fund. Alejandro Portes's essay (p. 401) is the major exception.

The book as a whole documents a long tradition of successful dispute mediation in the spirit of the current Contadora process and notes the relative absence of protracted warfare since the late nineteenth century. However, as Jorge Dominguez writes, the erosion of U.S. hegemony combined with the intrusion of the cold war and differential levels of economic development creates new threats to stability. The Steering Committee presents detailed recommendations for responding to a wide range of problems by revitalizing the OAS and other means of collective decision making—without further involvement of the United Nations. Unfortunately, these mechanisms are not able to cope with cold war issues, and the Reagan administration, suspicious of international organizations and unimpressed by specialized expertise, has seen fit to contravene a striking number of these suggestions.

The background papers on substantive issues of nuclear and conventional arms control, human rights, export performance, agricultural development, natural resource development, the environment, employment, population growth and movements, education, science and technology, and industrial policy are uniformly concise, imaginative, and informative. Written in 1981, they are somewhat dated, but this should not discourage readership. The essays describe the weakness of regional development schemes, point to the growing integration of export/import sectors of the larger countries into the world economy, and argue that countries with different levels of development require different public policies and ultimately changed alliances. Topics not covered include purely military considerations, the role of private voluntary organizations, and the impact of medium-to-long-term changes in political systems—including the United States'. Nevertheless, the broad scope of *Governance in the Western Hemisphere* is perhaps its greatest virtue. Those who criticize the work are likely to do so on philosophical rather than technical grounds. The Yudelman-Glade disagreement on the existence of physical resource restraints on agricultural production (pp. 324, 349) illustrates this point, as do the Werner Baer, William Glade, Kirk Rodgers, and Alejandro Portes critiques of strictly pro-growth, market-oriented, export-led, and capital-intensive development strategies.

In reading these essays, I am struck on the one hand by the changes in the region since I began to study Latin America 20 years ago: The OAS will soon have a majority of non-Spanish-speaking members; the real GNP has more than tripled since 1950; two-thirds of the more than 380 million people are now urban; three-fourths are literate; and life expectancy at birth has increased by 20 years since 1958. On the other hand, U.S. foreign policy, Portes's condemnation of a development model that leaves 49% of families in extreme poverty (p. 384), and urban industrial growth that worsens conditions of life for most city dwellers (p. 389) lead me to think that "the thing that hath been is that which shall be," and —some flawed and some failed revolutions aside —that there is no new thing under the Latin sun.

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Normative Theory

The Rise and Decline of Western Liberalism. By Anthony Arblaster. (New York: Basil Blackwell, 1985. Pp. viii + 394. $24.95.)

Anthony Arblaster has written an informative and thorough account of the history of liberal ideas and practices. In Part 1 of the book he presents a particularly clear and compelling picture of liberalism as a world view, an official way of seeing built upon certain ontological and epistemological foundations such as the separation of humans from nature and from one another, the separation of facts from values, and the connection of property to desire. Part 2 contains an ambitious presentation of the evolution of liberalism from the Reformation to the welfare state. Although much of this presentation is standard fare, it is very well researched and presented and includes some nonstandard figures in the litany of liberal voices: de Sade as well as Hobbes, Spinoza along with Locke, Wollstonecraft accompanying Shelley. Part 3, which focuses on twentieth-century liberalism, is the briefest and least interesting of the three parts. Here Arblaster attacks the easy targets of McCarthyism and the "end of ideology." He uses primary sources extensively and consciously tries to avoid sexist language and to point out sexist assumptions in theory and practice. His weaving together of literature and music with history and theory presents a view of the liberal corpus that is more extensive than usual.

A persistent theme in Arblaster's critique of liberalism is the repeatedly demonstrated connection between the liberal world view and its accompanying fear of and contempt for the poor. He makes an essentially moral critique of liberalism, showing the wide gap between its supposed respect for the individual and its blatant disregard for individuals if they do not measure up to capitalist standards of success. On another level Arblaster's critique is ontological, showing the inadequacy of the reliance upon the separate and distinct consciousness for an understanding of human sociality. On yet another level his argument is epistemological, attacking the fact/value distinction by showing that liberalism itself vacillates between a descriptive and a prescriptive account of human nature. Finally, it is also a political argument, showing that liberalism's individualist focus and affinity for "great man" theories of history are hostile to collective action.

Given the complexity of the analysis Arblaster provides, the flatness of his conclusions is disappointing. His critique of liberalism is itself constrained by the limiting conditions of the liberal arguments. He focuses on "the dark side of liberalism" in order to "balance" it (p. 347) and wants to preserve an "ambiguous attitude toward liberalism" because "the best of liberalism is too good to be left to the liberals" (p. 348). By emphasizing the way that liberalism betrays its own principles, he gives too much to those principles, which are after all complicitous in their own betrayal. The logic of his own argument suggests that liberalism did not simply go "off track" from otherwise sound principles, but was from the beginning based on "a defective and inadequate way of conceiving of human beings" (p. 349). If this is so (and I agree that it is), then can one start from liberalism and get where one wants to go? If Arblaster took his own argument more seriously he might be forced to look in fresh directions—perhaps toward feminism or some other philosophy of liberation—for a more adequate view of humanness, rather than relying on the tired old liberal formulas to somehow heal themselves.

KATHY E. FERGUSON

Siena College

Insight and Vision: The Problem of Communism in Marx's Thought. By R. N. Berki. (London: J. M. Dent & Sons, 1984. Pp. x + 204. $19.95, cloth; $9.95, paper.)

This is a delightful and disturbing book. Berki says he had fun writing it, and his enthusiasm manifests itself in a lively, witty style. Yet he also examines issues crucial to Marx and marxism with painful honesty. His two concerns are to show that communism is central to Marx's thought, and communism poses profound problems for that thought. Although Berki seldom explicitly addresses secondary literature (a point to which I return), his insistence that "ultimately communism is the only thing that is important about Marx's thought" allows him to imply its inadequacies (p. 1). He concedes, as most interpreters argue, that Marx says little about communism *as an ideal,* but maintains that this cursory idealized vision does not exhaust Marx's views on communism. Berki argues that Marx has two views of communism, as a vision of an ideal society and an insight into the real movement of history. Interpreters tend mistakenly to emphasize one or the other side: Marx is either a visionary revolutionary inexplicably fascinated by empirical realities or an insightful social scientist embarrassingly concerned with moral ideals. The problem of communism for Marx (and his interpreters) is how to fuse these two views. Vision and insight, ideal and real—ultimately, communism and capitalism —must come together.

Berki provides a developmental account of Marx's views on communism, illustrating the tension between vision and insight throughout. Beginning with the largely visionary early writings, he argues that Marx condemns many features of capitalism *before* he designates them as capitalist. According to Berki, this condemnation is based upon conceptions of human nature entailing universal freedom, species-being, and substantive rationality. These fundamental moral objections to capitalism as somehow inhuman persist when Marx begins (as early as 1844) to develop his analysis of capitalist economics. However, as Marx strengthens his empirical side, the tension between his original vision and his subsequent insights becomes acute. Berki illustrates this tension through Marx's dualistic concepts of the proletariat as a class in itself (insight) and for itself (vision) and revolution as continuation of capitalism (insight) and complete transformation of it (vision). Here, Berki asks: Why would a worker want human emancipation instead of satisfaction of his/her own concrete wants (p. 65)? Is revolution an adequate way of arguing that communism is *here,* that it is imminent, the real movement of society (p. 85)?

Berki regards the *Grundrisse* as Marx's attempt to fuse vision and insight in response to these questions. He considers it to be the pinnacle of Marx's thought, and his remarks on it are also the pinnacle of his book. He treats it as a *"phenomenology* of capital in the genuine Hegelian sense, the thinking *development* of capital to the point where it is truly revealed as what it is, the communist essense of mankind" (p. 96). He demonstrates how communism is already here under capitalism by discussing their dialectical fusion in humanity's struggle against external nature, the generation of universal freedom, and the progressive requirements of production. He concludes: "To be a 'communist' in Marx's fully matured, perfectly fused dialectical understanding means no more, and no less, than to be a *very profound* 'capitalist' " (p. 118). However, Berki correctly argues that this Hegelian *Aufhebung* is only a pyrrhic victory: If communism is already here (as Marx knows it is not), then revolution is superfluous, and if revolution is necessary (as Marx knows it is), then communism remains an ideal opposed to reality. Marx responds to this continuing tension in *Capital* by demoting communism to a refined, rationalized capitalism, to freedom built upon necessity. Yet Marx still wants and needs to separate vision and insight, as he reveals in the *Critique of the Gotha Program* by distinguishing lower and higher phases of communism. He fails to show any developmental relation between them, however, and the problem of communism remains unresolved.

Berki's analysis is disturbing because he confronts contemporary Marxists with crucial questions: Can communism be established as a revolutionary departure on the basis of insight alone without recourse to visionary elements? If so, will insight conform to vision (p. 19)? And, I would add, if not, is vision arbitrary? Berki concludes that Marxism requires both vision and insight, but he reconstrues their relationship. He could and should develop this argument with explicit reference to contemporary schools. His message, I think, would be that communism, whether that of critical theorists, structuralists, or poststructuralists, cannot be defined negatively. "Morality must be read into human nature for communism to make any sense at all" (p. 73). Vision must transvalue capitalism philosophically and politically, precisely because the communism of insight that already exists requires improvement. But Berki also suggests that it is naive and dangerous to superimpose vision on insight. Instead, he would infuse the communism of insight with the communism of vision. What he leaves obscure is the status of this vision. His critique of insight precludes relativist or reductionist defenses of it, but he does not consider realist ones. Can a Marxist be a moral realist, or is communism, as Berki assumes, ultimately a theological world view?

NANCY S. LOVE

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Marxism: The Science of Society, an Introduction. By Kenneth Neill Cameron. (South Hadley, Mass.: Bergin & Garvey Publishers, 1985. Pp. xvi + 222. $25.95.)

Cameron offers his reader an introduction to Marxism that discusses Marx's and Engels's thought, maintains that current communist countries are moving toward the realization of Marxist goals, and applies Marxism to present problems. Following a poem to the Rosenbergs, a Preface, and a personal statement, the book is divided into two main sections. The first two-thirds treats the ideas of Marx and Engels: the roots of Marxism, the ubiquity and centrality of class struggle, the crucial role of economic activity, the analysis of capitalism, the projection of communism, and the theory of history that grows from and unites these central ideas. The last three chapters discuss how Marxism helps the reader understand the status of women in society, the relations of ideology and literature and class, and the dialectical materialist character of modern natural science. A Professor Emeritus of English and author of studies of Shelley, Cameron presents himself as a dedicated Marxist who thinks that Marx and Engels usually were accurate—in essence if not in detail—but that "Marxism does need to be revised" (p. xi) to take into account modern circumstances and knowledge.

Cameron's book has two striking characteristics. Despite his own commitment, it is oddly bland. Primarily descriptive, it contains few sustained or rigorous arguments. Core concepts such as class and science are left tranquil, unexamined and (apparently) unproblematic. Marx's intellectual passion does not show through. The book is also anachronistic. Published in 1985, it reads as though written in the 1930s in Britain or North America. The extensive debates of the past 50 years seem mostly to have passed by Cameron. The notes refer to few secondary sources, and the text alludes to few of the many controversies in Marx interpretation that have excited scholars and activists. Similarly, Cameron essentially ignores Marxist writings and ideas that have become current in the past half century. Marx's *Grundrisse* is mentioned not at all, his *1844 Manuscripts* once, *The German Ideology* infrequently. Alienation and estrangement are in neither the index nor the text. Even when discussing issues of great contemporary importance—the status of women, ideology, and literature—Cameron eschews recent work, Marxist and non-Marxist. When he discusses modern natural science and sociobiology, Cameron employs recent scientific writings, but the Marxist ideas he uses in his discussions date from an earlier time.

The Marx scholar may find a few aspects of Cameron's *Marxism* of interest: his clear description of value, his careful restatement of the distinction between base and superstructure, and his argument that feudalism is a prevalent historical stage (not merely limited to medieval Europe) and slave societies (e.g., the Roman Empire) are correspondingly rare. (Surprisingly, Cameron contributes little of interest on Marxism and literature.) The student seeking a single introductory interpretation of Marxism will, I think, not be well served by Cameron's book. Essentially out of date decades before its publication, it does not cover Marx's *corpus* adequately, it does not stress points of scholarly (or practical) contention, and its lax argumentation is not a model to be emulated by the beginner. In tandem with other interpretive texts, however, Cameron's book might be stimulating, precisely because anachronistic: Its point of view is almost certain to differ from the others, sometimes in ways likely to spur thought and debate (such as the Marxist character of the Soviet Union or the meaning of science to Marx). In general, however, both student and scholar should look for other introductions to Marxism.

PETER G. STILLMAN

Vassar College

Hegel's Critique of the Enlightenment. By Lewis P. Hinchman. (Gainesville: University Presses of Florida, 1984. Pp. xvi + 299. $25.00.)

For at least 15 years a number of English-speaking political philosophers have been engaged in a serious attempt to come to terms with Hegel. The appearance of this thoughtful study is one sign that this movement is now reaching a kind of maturity. Although he focuses principally upon the arguments of Hegel's *Phenomenology of Spirit* and *Philosophy of Right,* Hinchman demonstrates an impressive command of the entire Hegelian *corpus.* His exposition of the central concepts of Hegel's philosophy is accurate and clear. He relates those concepts to Hegel's philosophical background with apparent ease. Even if in some respects Hinchman essentially follows in the footsteps of earlier commentators, most notably Stanley Rosen and Charles Taylor, the confidence with which he strides through the thought of this notoriously difficult philosopher is a sign of real intellectual achievement.

Hinchman reads Hegel's entire philosophy as an attempt to resolve the problem of alienation. On a philosophical level alienation is expressed in the dualism that underlies most of modern philosophy from Descartes through Kant. On the level of immediate experience it is manifested in a feeling of "personal fragmentation" (pp. 97, 116) or, as Hinchman puts it at one point, in "the inability of the individual completely to identify himself with the world of culture he inhabits" (p. 122). Alienation is a product of separation between self and world, of the kind of critical reflection upon the world that goes by the general name of enlightenment. Hence the book's title, as well as its central theme that Hegel should be viewed as a critic of the enlightenment, broadly conceived.

Hinchman recognizes that Hegel did not simply reject enlightenment. In fact, one of Hegel's aims was to show that enlightenment and alienation are necessary aspects of the development of the absolute (p. 42). But Hinchman argues that Hegel's deepest aim was to overcome the disintegrative and dualistic features of an "enlightened" society. Conceived in the broadest terms, then, Hegel's project was to recapture the sense of wholeness and community that had characterized the ancient *polis* before the beginnings of enlightenment (the origins of which can be dated as far back as the Peloponnesian War) without sacrificing the freedom that is made possible by critical distance from one's community and its rules and norms (p. 217).

Hinchman's account of this formulation of the problem is sympathetic, even glowing; but his assessment of the "solution" proposed in the *Philosophy of Right* is mixed. He praises Hegel for adopting his famous and much-criticized view that the state should be seen as the fullest expression of human freedom (pp. 233-234). Yet he attacks Hegel's vision of political society for allowing too little room for individual freedom and political participation. Because of Hegel's confining notions of the family and the estates as well as his strongly antidemocratic biases, Hinchman concludes that "he has come nowhere near reconciling ethical life and subjective freedom" (p. 232).

This criticism is certainly correct if we assume that the reconciliation must take place on the level of immediate, personal experience; and it is easy to sympathize with Hinchman's complaints about the anachronistic character of Hegel's conceptions of the family, estates, corporations, and popular sovereignty. Yet it is difficult to see how Hegel could have satisfied Hinchman's requirements. Freedom depends upon critical distance from one's community, while the sense of personal wholeness he wishes to recapture flows from unselfconscious identification with that community. It is not evident that the combination of statism with citizen participation toward which Hinchman points us would lead to the happy result he envisages. Hegel attempted to combine freedom and community only from a philosophical standpoint and not, as Hinchman sometimes leads us to believe, on the level of immediate experience. His practical skepticism should perhaps instruct us no less than his theoretical boldness.

DAVID JOHNSTON

Yale University

Democracy East and West: A Philosophical Overview. By Howard P. Kainz. (New York: St. Martin's Press, 1984. Pp. vi + 152. $22.50.)

Political scientists might be misled by the title of this book: This is not a comparative study of democracies. Instead, as the subtitle indicates, the volume provides a philosopher's view of the state of democratic theorizing. The survey covers 10 chapters with topics as wide-ranging as "Equality *vs* Liberty?," "The Economics of Democracy," "The Two Party System: Some Qualified Advantages," and "Is Democracy the Best Form of Government?" Kainz, best known for Hegel scholarship, clearly and competently links major figures in the Western tradition of political philosophy and evaluates the contributions of contemporary theorists. This successful effort to rescue the concept of democracy from imprecise usage is guided by Hegel's view of philosophizing as "making explicit the second-order empirical 'givens' (ideas and values) which have already matured in the body politic and are ready for the 'picking' " (p. 10).

The rich tapestry of democratic theory certainly merits a study of this kind; yet the project's very advantages are also its weaknesses. Although this sort of conceptual overview is invaluable (especially, one can imagine, for graduate students), and although the connections made between different theorists are suggestive, the book's format necessarily generates an all-too-brief analysis of each concept or issue. When reading this book, one longs for a volume-length treatment of each chapter's contents. Of course, this reaction is not so much a criticism of the work as it is a compliment to Kainz's engaging style and cogent insights. At times, because of the sheer scope involved, the volume lapses into doxology, and one is tempted to see which relevant theorists from the discipline of political science are not discussed. However, the omission of references to some standard works in the field is more than compensated by the inclusion of the unexpected: theologians' and anthropologists' insights are woven into the work. Two of the most provocative chapters, "Fraternity—An Inquiry" and "Religion in a Democracy," contain material often neglected in studies of this kind.

The North American model serves as the conceptual touchstone in Kainz's analysis of the theory of democracy, but the comprehensive nature of the study leads him to refer occasionally to other models as well (European and Israeli versions appear most frequently), and to investigate the democratic claims of marxist regimes. Throughout the book a persistent and underlying comparison appears between U.S. liberalism and varieties of Marxism. At times, the analysis appears to reflect an ideological bipolarity: For example, in the penultimate chapter Kainz provides three possible scenarios for the future of democracy in the international arena, and each scenario rests on the assumption of ideological bipolarity. A more explicit discussion of ideological multipolarity in international relations might have been helpful here.

Drawing on Hegel's view of the modern state as a dialectical polarity of subjective (individuals) and objective (institutions), Kainz suggests that democracy can finally best be understood as a series of polarities. The polarities of populism and elitism, bigness and smallness, liberty and equality, religion and secular life, and libertarian and social democracy are met by a second-order polarity that refers to the interpretation of the other polarities, the adversary and the consensual interpretations. The vitality of democracy is, Kainz argues, in the constructive opposition of these polarities. The final polarity of approaches to interpreting the first order polarities certainly merits additional consideration, for it raises a "limits of toleration" question.

Although this is not an exciting volume, it is a congenial one. Kainz's conceptual fluency and his rejection of simplistic formulations of democratic theory are welcome contributions.

VIRGINIA LEWIS MULLER

University of San Diego

Relativism and the Natural Left. By William P. Kreml. (New York: New York University Press, 1984. Pp. xiv + 182. $10.00, paper.)

As the United States enters Ronald Reagan's second term, the fate of the New Left seems bound to scholarship. William Kreml's new book, for example, attempts to fashion a new philosophy of politics to revive radicalism. The result is neither intellectually nor politically satisfying.

Kreml argues that the political philosophy of the future will be based on relativism understood as an acceptance of a variety of human natures. Modern philosophy, he suggests, has replaced the ancient logical distinction between skepticism and rationalism with a psychological division between relativism and rationalism. The former concept has taken a random and restricted form; modern rationalism, on the other hand, offers rigid and flexible alternatives. Kreml believes that random relativism has led to nihilism that in turn joined with rigid, analytic rationalism to bring forth positivism. His hopes, however, lie with a "restricted" relativism that is compatible with a flexible "soft rationalism." The meaning of these terms remains elusive until his chapter on psychology. In any case, their grounding in evidence drawn from the history of ideas is none too deep.

The psychology chapter distinguishes two familiar types. The conservative-analytic personality needs strict lines and boundaries, whereas the radical-synthetic personality accepts the flux of reality without harsh demands for distinctions. In the latter category, Kreml further distinguishes the Logical Left, who are concerned with economic interests, from the Natural Left, who are the true radicals and defenders of relativism. The third chapter then criticizes the social sciences for not adopting "soft rationalism."

The final two chapters directly concern politics. The fourth chapter is largely an attempt to restructure the Left by attacking Marx's economism in favor of a left-wing Hegelianism. The Marx appearing here is a straw man. Kreml seems unaware that the last few decades of scholarship have found in Marx's early writings a psychological theory similar but deeper than the one propounded in these pages.

Unfortunately, Kreml's treatment of other philosophers also lacks the influence of primary or secondary literature. This objection is not merely pedantic, for such knowledge might have precluded some curious interpretations. Kant, for example, is branded a skeptic and "the quintessence of antirational thought," perhaps because neither the categories of the understanding nor the transcendental method is discussed. The Hegel who proposed a single concept of will that approaches the Absolute at the end of history becomes in these pages a "soft rationalist" who accepts a variety of human natures. Almost every other thinker in Western history merits at least a paragraph, but none receives adequate treatment. After a time, the parade of names becomes irritating rather than impressive.

The political prescriptions of the book are even less convincing. Kreml states that he has created an "emergent" political philosophy, however, in a country moving quickly toward conservatism, some evidence should have been offered to support this assertion. Indeed, a few remarks suggest that Kreml's ties to the Left are rather weak. His praise for that flatterer of power, Thrasymachus, and for "natural hierarchies within the human population" are hardly compatible with egalitarianism.

In general, this book offers little that is new to either the history of ideas or the study of contemporary political theory. Critical thought may mark the conservative-analytic personality, but clear definitions and relentless logic provide the foundation of an accurate understanding of reality. To judge by this book, abandoning the hard edge of analysis permits indulgence in a confused nostalgia that refuses to come to grips with the challenges of the present and future.

JOHN SAMPLES

Southwest Texas State University

The Nature of Political Theory. Edited by David Miller and Larry Siedentop. (New York: Oxford University Press, 1983. Pp. 261. $39.95.)

One expects a good deal from a book with so impressive a title. Fortunately, this collection of essays produced in memory of John Plamenatz satisfies many of one's expectations. David Miller introduces the book with the suggestion that political theorizing is a special activity: an activity distinct from either political philosophy or intellectual history. Political theory is, Miller asserts, a blend of the philosophical, the empirical, the moral, and the rhetorical.

This claim receives ample support from the essays in this volume. Indeed, Alasdaire MacIntyre's essay on "The Indispensability of Political Theory" takes Miller's assertion one step further. MacIntyre contends that humans are natural political theorists because only as such can they constitute their world. Hence, from MacIntyre's perspective, what separates the layman from the professional theorist is the theorist's greater precision and self-consciousness. Otherwise, both are equally involved in the essential task of attributing meaning to the world. Although MacIntyre's essay does not break new ground, it is a useful summary of one variant on recent antipositivism. It is, as well, a nice starting point for the essays that follow.

Both David Miller and Larry Siedentop pick up the threads of MacIntyre's work. Miller does so in order to show that a philosophical analysis of most political concepts entails the development, witting or not, of a political stance. In particular, Miller argues that any consideration of such concepts as "justice," or "democracy," or "freedom" pulls one into a partisan pose. It is a view confirmed by both Peter Jones's essay on our commitments to democracy, equality and majority rule and Brian Barry's analytic piece on the relationship between "nationalism" and "individualism." All three essayists recognize the partisanship of political theorizing.

On a parallel tack, Siedentop argues that the concept of the "state" is an especially modern notion, one uniquely related both to the norm of political equality and to the idea of individualism. In short, the concept of the state is, for Siedentop, both the product and the producer of a particular form of life. Siedentop's stress on the ideological character of some of our political vocabulary, and more importantly his emphasis on the constitutive role of our political ideas, is persuasively supported by Robert Wokler's essay on "Rousseau and Marx." Wokler argues that Marx failed to appreciate Rousseau, and, in particular, he failed to share Rousseau's sensitivity to the role that ideas play in structuring our political world. Wokler goes on to suggest that had Marx read Rousseau more carefully, Marxist theory would have been both more subtle and more persuasive.

However, the theme of John Gray's "Political Power, Social Theory, and Essential Contestability" runs directly counter to the views parlayed by Miller, MacIntyre, Siedentop, et al. In a thoroughgoing assault on William Connolly's and Steven Lukes's respective conceptions of power, Gray searches for the leverage with which to challenge the utility (and meaningfulness) of that much used phrase, "essential contestability." In other words, Gray doubts the claim that social theory is inherently value laden; consequently, he contends that "if social thought is not to be lost in a no-man's-land of political controversy, its practical character needs to be denied and its direct links with moral and political life severed" (p. 101).

Three further essays complement those already mentioned. Alan Ryan's "Hobbes, Toleration, and the Inner Life" argues that Hobbes was rather "liberal" in his readiness to respect the private beliefs of individual citizens. Geoffrey Marshall uses his essay on "The Roles of Rules" to reveal the complexity of the concept of a "rule."

And, Steven Lukes, in response to the question "Can the Base Be Distinguished from the Superstructure?" suggests that G. A. Cohen's recent defense of Marxist thought is incomplete.

This is a significant collection of essays that effectively represents both the diversity and the vitality of contemporary political theory. If little new philosophical ground is broken, the old turf is well-tended and handsomely groomed.

J. P. Geise

Clarkson University

What Should Political Theory Be Now? Edited by John S. Nelson. (Albany: SUNY Press, 1983. Pp. xv + 607. $35.00, cloth; $12.95, paper.)

This book is the product of the 1981 Shambaugh Conference on Political Theory held at the University of Iowa. Eighteen essays are included, three by the editor himself.

The question, as many might suspect, is never really answered by any of the participants. In that sense, this collection may well represent the state of academic theorizing in the United States, in that far too often ponderous questions are asked that presuppose ponderous answers.

It is unfortunate that so many of these theorists succumb to the temptation to pontificate and polemicize. Most of the essays contained in this too-large volume are not representative of either the best efforts of the individual theorists, or more generally representative of good theorizing. Because the book is such a sprawl, comments on all of the essays must be made somewhere else. But there are a few observations that must be made here.

Some of the essays are valuable. But these contributions, made by participants such as John G. Gunnell ("In Search of the Political Object: Beyond Methodology and Transcendentalism") and William E. Connolly ("The Dilemma of Legitimacy"), although welcome anywhere, can be found elsewhere, sometimes in more developed and mature form.

Because of the mandate of the conference, one of the worst temptations succumbed to in this volume is when contributors present vitae of their individual careers. This is particularly unfortunate in the case of James M. Glass ("Structures of the Self: Reflections on Where Political Theory Should Be Now"), who cites himself eight times (Freud himself only is mentioned three times) in an 18-page essay, each time uncritically. His major point is that psychoanalysis ought to be taken seriously by political theorists.

Some of the theorists use the occasion to try to describe what they think the state of the subdiscipline is in regard to political science. Robert Boothe Fowler ("Does Political Theory Have a Future?") evokes most of the fears academic theorists are expressing these days, but one must ask whether it is necessary to describe yet again how marginalized and miserable theorists feel about themselves in relationship to the discipline. Such heaviness should be more leavened with humor, or at least hope.

A major problem of this book is its general incoherence. Academic theorists of every stripe contribute something, so there are essays on contemporary theory, the Greeks, Heidegger's Nazism, democracy and liberalism, the science of politics, socialization theory, Marxism, and other approaches (remarkably absent is any systematic essay on feminist political thought, perhaps the only completely new development in the field in the past two decades). The overall effect is like that left should a news commentator have a member of the KKK rebut a Black Panther on issues of voting rights. What is sought is balance, what appears is silly.

Finally, Nelson includes far too much material (165 pages) by himself, material that represents the worst tendencies to be found throughout this book. When one reads his essays, one is struck by how undisciplined the writing is. Nelson's essay on political rhetoric especially is innocent of any basic principles of clear writing, surely the prerequisite for any rhetoric.

What should political theory be now? If the answer is a volume such as this, perhaps it would be better not to ask.

Thomas L. Dumm

Amherst College

Liberalism at Wits' End: The Libertarian Revolt against the Modern State. By Stephen L. Newman. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 184. $19.95.)

Libertarian John Hospers declared in 1971 that libertarianism was an idea whose time had come. In the subsequent years a flurry of activity has surrounded this antistatist movement (publications, foundations, a party), and Stephen Newman's study is a response to this surge, raising questions about the seriousness of the libertarian revolt. The strength of Newman's book is his attempt to treat libertarian ideas seriously and fairly while not becoming an uncritical enthusiast. In fact, he sees the libertarian alternative as "gravely flawed" but deserving "to be treated seriously, for its defects have implications that touch on the philosophical foundations of the American polity" (p. 16).

Newman does a creditable job in identifying the major contemporary figures allied to the movement (Hayek, Friedman, Hospers, Rothbard, Nozick and, of course, the late Ayn Rand). He details the philosophical roots of modern libertarianism by tracing the ideas of Locke, Godwin, and the American anarchist Lysander Spooner. Not surprisingly though, Newman's look at libertarian ideas centers on its Lockean heritage. This attachment to Locke, according to Newman, not only provides the fountainhead of libertarian thought, it also supplies the major source of dispute among libertarians and, to its detriment, a blindness about political and economic reality.

Newman's most insightful work is found in chapters 5 and 6, where he analyzes Locke's notion of the State of Nature. Apparently, a case can be made that Locke had two theories about the State of Nature: One in which individuals are rational, peaceful, "having no incentive to break natural law"; and a second, after the invention of money, where "the common stock has been transformed into relatively few private hands" and where "people will have reason enough to steal" (p. 96).

Libertarians disagree about this pre-state condition, and therefore over the need for, and the proper role of, the state. Some libertarians, like Rothbard, ascribing to the first condition, want to return to a stateless situation where economic rationality alone will produce social harmony. Others, like Hospers and Nozick, reject the views of anarchocapitalists and agree with Locke that a limited, nightwatchman state, or miniarchy, is needed.

But turning from this intrafactional dispute, if America is really the preeminent Lockean society with its antistatism, individualism, and capitalism so deeply rooted, why does libertarianism appear at its wits' end in America? Newman suggests the following answers. First, although libertarians reaffirm familiar themes, they fail with the many by "elevat[ing] the stuff of everyday life to the level of dogma" (p. 33). Americans are comfortable with their Lockean themes, but not when they are relentlessly pursued to their ultimate conclusions (e.g., private police agencies). Second, Newman feels that Locke was addressing a set of uniquely European problems associated with "aristocratic privilege, enforced conformity by the established church," and support for representative government (p. 161). But by taking Locke's ideas out of their feudal context, American liberalism has created a dilemma for itself by allowing the market to supercede the political. That is, by privatizing our social existence we have created a serious political dilemma that avoids all notion of public goods or collective purposes (consumership over citizenship). As a result, American politics, following Lowi, is simply the ability of private, organized groups to use the leverage of state power to satisfy their demands, as if public goods are simply the sum total of individual preferences. Third, Newman feels that any libertarian order would fail because it underestimates the disruptive forces of politics "fueled by emotions" (p. 125). The libertarian fantasy involves an escape from politics by recreating in thought the Lockean State of Nature. But that too was a fantasy.

In sum, Newman's book provides an excellent introduction to the philosophical arguments and shortcomings of modern libertarian thought, although one suspects that committed libertarians will not be deeply dissuaded by his arguments.

LAWRENCE J. CONNIN

University of Toledo

Justice Oliver Wendell Holmes and Utilitarian Jurisprudence. By H. L. Pohlman. (Cambridge, Mass.: Harvard University Press, 1984. Pp. 223. $18.50.)

In undergraduate courses in constitutional law, Justice Holmes's sharp dissents in *Lochner* v. *New York* (1905) and *Adkins* v. *Children's Hospital* (1923) often provoke discussion about dramatic changes in jurisprudence from the nineteenth and early twentieth century. In such discussions, it is commonplace for students to be informed that Justice Holmes was at the cutting edge of legal thought at the turn of the century; that he helped to discredit the "myth of mechanical jurisprudence" and the "oracular theory of judicial decision-making." Students are taught to view Holmes's contributions to jurisprudence as representing a sharp departure from the past. This celebratory view of Holmes as innovator pervades the scholarly literature as well as the classroom discourse. The literature tends to portray Holmes's jurisprudence as radically American in its pragmatism, sweeping away the dusty legal fictions of the English common law. In his provocative reexamination of Holmes's legal ideas, H. L. Pohlman attempts to demonstrate that Holmes was more indebted to John Austin and the other nineteenth-century English utilitarians than either he or subsequeent scholars have acknowledged. Although Pohlman's thesis is not entirely original, it is very persuasively argued.

The author recognizes at the outset that "[s]ince so many commentators have written so much on Holmes's jurisprudence already, it seems hard to justify yet another study" (p. 9). Lest the reader think that this is to be merely another foray into legal or historical esoterica, Pohlman states his dissatisfaction with contemporary jurisprudence which, he claims, "is increasingly becoming more academic, abstract, and prescriptive" (p. 9). Implying that Holmes would himself revolt against the current tendency, Pohlman makes his case for another attempt to properly understand Holmes's legal theories.

Pohlman is not out to discredit Holmes nor in any way to detract from his reputation as an original thinker. Indeed, the reader becomes aware that the author's purpose is to give Holmes's reputation "added status and security" (p. 177) by "placing his thought within the context of utilitarian jurisprudence" (p. 177).

To his credit, Pohlman gives short shrift to general and superficial similarities between Holmes's thought and that of Austin and the other utilitarians. Rather, he offers a systematic, rigorous, and thorough examination of logical relationships between Holmes' and the utilitarians' definitions of law, theories of legal development, legal obligation, liability, and judicial decision making. Through careful exegesis and exacting logic, Pohlman argues convincingly that many of the differences between Holmes's jurisprudence and utilitarian orthodoxy are more semantic than substantive. For example, the author shows how Holmes "came to his famous definition of the law as the 'prophecies of what the courts will do in fact,' not by applying new scientific or pragmatic principles to the law. . ." (p. 60), but rather by refining Austin's conception. "Though Austin defined laws as the commands of the sovereign, he often used judicial enforcement as the criterion of what rules were sovereign commands" (p. 60). Because Holmes the positivist "detested fiction in the law" (p. 60), he simply eliminated Austin's philosophical definition and adopted Austin's criterion, or what we would call his "operational definition." Thus Holmes's realism was not revolutionary; it was simply the extension of the "linguistic reductionism" (p. 130) espoused by the utilitarians.

Although Pohlman's three chapters on explicitly legal concepts will be the primary focus of law students, political theorists as well as epistemologists will no doubt find the chapter on "Reality, Ideas and Language" of most interest. Here Holmes's philosophical premises are examined relative to those of the utilitarians. The reader discovers that, although there are certain differences in terms of substantive conceptions, Holmes and the utilitarians "adhered to a common philosophical method" (p. 143), that is, inductive reasoning based on the facts of common experience.

Having demonstrated the connections between Holmes's ideas and those of the English utilitarians, in his concluding chapter Pohlman undertakes a neo-Holmesian (properly understood) critique of contemporary jurisprudence. He finds it wanting for the very same reasons that both Holmes and the utilitarians in their epochs criticized legal orthodoxy. Pohlman finds current jurisprudence excessively prescriptive, abstract, idealistic, and impractical. Although his argument is meritorious, this concluding critique of contemporary legal thought is the least successful part of the book. Perhaps it is because Pohlman simply takes on too much (e.g., the theories of Wechsler, Dworkin, and H. L. A. Hart) that the discussion seems conclusory and somewhat superficial.

On the whole, Pohlman has produced a thoughtful, thorough, and highly relevant reexamination of Holmes's jurisprudence. Although he clearly identifies with Holmes intellectually, he is careful to avoid writing another in a long series of panegyrics. Rather, he provides a very meaningful contribution to the voluminous body of Holmesian scholarship.

JOHN M. SCHEB, II

University of Tennessee

Sophia and Praxis: The Boundaries of Politics. Edited by J. M. Porter. (Chatham, N.J.: Chatham House Publishers, 1984. Pp. 148. $12.95, paper.)

The book consists of five essays plus critical commentaries that try to "understand and resuscitate the political dimension of human existence" and to give a "philosophical defense of a politics conceived as a public and reasoned collective action" (p. viii). The authors think that the contemporary tendency to split wisdom (*sophia*) from action (*praxis*) has led to a situation where a politics of common sense and citizen involvement has largely been replaced by the image and reality of politics as a technical activity for specialists. By exploring the "various ways that *sophia* and *praxis* are related in a true politics" (p. viii), it may be within reach to unearth the primordial meanings of politics along with the original human experiences that make politics possible. The book rests on some fundamental premises, namely, that persons become fully human by virtue of their capacity to be political; that politics is the necessary precondition for the development of moral character for the individual and the community; and that politics rightly understood expresses the life of reason only when *sophia* and *praxis* maintain their organic, living relationship.

The essays as a whole explore the "boundaries of politics" from diverse philosophical and historical perspectives, and further, they may be conceived as theoretical responses to questions about the connections between knowledge, action, and politics. For example, how and why did politics originate, and what makes politics a distinctively human and vital experience? Is the link between politics and ethics a primordial one, and did the ancient Greeks develop ideas about *sophia* and *praxis* that are inseparable from politics even if the contemporary eye fails to perceive these ideas clearly? Have we as moderns shaved away the intrinsic moral dimensions of political deliberation and action because we have been subject to historical, cultural, and epistemological forces that have made us blind to the place of politics in human affairs? The essays respond to other questions as well. How do *sophia* and *praxis* in politics differ from other modes of cognition and activity, for example, from work, technical rationality, and stimulus response behavior? What role does political language or rhetoric play in discerning moral standards and applying these in practice, and how does the exercise of reason in politics differ from other modes of reflection? And if politics inherently involves deliberation about moral values and ends, is it possible to establish criteria to rank order qualitative judgments? What does politics presuppose about the nature of thought and action, and how does political activity unite the apparent duality of man as knower and actor? Is it the case that statesmen as opposed to politicians embody most fully the capacity to translate *sophia* into *praxis,* and if so, what factors account for their ability to lead in a truly visionary way while educating the citizenry to make independent judgments?

A few words are in order about the individual essays. Jürgen Gebhardt explores the existential and moral possibilities the ancient Greeks' thought intrinsic to politics, claims that political philosophy originated as an attempt to give symbolic form to the dilemmas of political experience, and shows through close attention to Greek political language the implicit unity of *sophia* and *praxis* as well as their primordial meanings. F. M. Barnard offers a fascinating and precise conceptual exploration of the nature of individual and political action, and he persuasively argues that politics cannot be understood in terms of mechanistic or natural processes because it rests on the capacity to give reasons. Political action intrinsically involves *sophia* and *praxis* as distinctive traits, and if politics is conceptually bound to discourse about reasons, values, and goals, it follows that practice separated from thought makes politics utterly meaningless. George J. Graham, Jr. and William C. Havard, Jr. focus on the contemporary split between *sophia* and *praxis* that they trace to scientific epistemology, to thinking in terms of subject/object dualities, to the separation of data-oriented from evaluative modes of discourse, and to the downgrading of political rhetoric. They rely on Aristotle's categories to formulate the dynamic connections between knowledge, deliberation, and action, and they argue for the centrality of political rhetoric as the medium by which knowledge becomes action, particularly as it pertains to the activity of statesmen.

Thomas E. Flanagan surveys the theories of F. A. Hayek and claims that the contemporary era is dominated by "constructivist" thought patterns that view society as akin to an organization that can be built and rebuilt once the human mind grasps the nature of its rules and laws. Against this vision, Flanagan following Hayek asserts that society is an order of evolving rules that grow from spontaneous human interactions, and further, that these rules cannot be subjected to massive tinkering by man without disrupting the deep-seated presuppositions that provide the necessary framework for *sophia* and *pracis* in a historical community. Constructivism undermines politics as a deliberative activity and makes politics the preserve of the rationalist technician. The final essay by Athanasios Moulakis interprets the decline of the classical interplay of *sophia* and *praxis* from the standpoint of evolutionary trends in Western culture that glorify work as the highest aim of life and as the truly distinctive quality of the human spirit. He claims that work in itself is without meaning; it provides no intrinsic values and goals, and it undermines the idea that *sophia* and *praxis* can alone provide authentic ends for life.

Sophia and Praxis is a book worth reading.

HAROLD SARF

San Francisco State University

The Future of the Democratic Revolution: Toward a More Prophetic Politics. By Neal Riemer. (New York: Praeger Publishers, 1984. Pp. xii + 307. $39.95.)

In this book Riemer suggests the paradigm of "prophetic politics" for the interpretation and guidance of political activity in the world. Approximately a third of the book is devoted to the examination of the weaknesses and merits of what he sees as major competing paradigms. He argues that the Machiavellian position recognizes many of the realities of politics but fundamentally threatens the achievement of international community and cooperative order. Utopian models, here represented by the ideas of Marx, Skinner, Plato, and Dostoevski's Grand Inquisitor, project the possibility of a better world but fail to recognize the ineradicable nature of conflict in society and to provide acceptable means for resolving differences among individuals. Liberal democratic politics receives better treatment from Riemer. It has provided for a continuing workable balance between individual liberty and the needs of the community at large. Yet Riemer finds this approach wanting in vision and creativity. Liberal democratic politics slides too easily into compromises and a short-term vision. Riemer argues that in terms of higher ethical standards and more creative approaches toward improving the human condition the prophetic paradigm is superior to the Machiavellian, utopian, and liberal democratic positions.

The remainder of the book is devoted to the definition of the prophetic paradigm and discussion of how it can be applied. Although he considers very briefly Greek philosophy, natural law, and the Enlightenment, Riemer clearly sees the prophetic values and messianic vision of the Sinai Covenant and the rules and processes of constitutionalism as the major ingredients of a prophetic politics dedicated to "the standard of a superior universal order" (p. 114). Riemer believes that prophetic politics must be future oriented and must maintain a critical stance toward contemporary governmental policies dealing with such worldwide issues as peace, human rights, economic well-being, and ecological balance, and much of the rest of the book is given to his consideration of alternatives for resolving these problems. One of the most interesting parts of this effort is his discussion of "prophetic constitutional breakthroughs," instances where Riemer claims that the ethical, empirical, and prudential standards of a prophetic alternative to a problem have been satisfied. He argues in some detail that Roger Wiliams's articulation of the case for religious liberty and Madison's argument for the balance of local liberty and national direction constituted legitimate prophetic constitutional breakthroughs. In the case of Williams, the bloodshed and bitterness resulting from dogmatic attachment to the one true religion were assuaged. And Madison's ideas rationalized the viability of a democratic republic encompassing a large geographical expanse. It is, however, this discussion of the fundamental theoretical changes that suggests perhaps the greatest difficulty with Riemer's effort at paradigm construction.

The basic problem with this book is that Riemer fails to provide a believable transition between the prophetic politics paradigm and the problems that he sees it being used to attack. The prophetic paradigm as presented by Riemer is in fact narrowly constrained to the Western Judeo-Christian, Liberal constitutional heritage. Even more confining is the fact that all of his specific examples of breakthroughs of a prophetic nature are American. Only once, where later in the book he allows eight lines to mention the European Common Market (p. 242), does he suggest an example of a non-American prophetic constitutional breakthrough. Yet the problems that Riemer sees as needing immediate attention are worldwide in scope, as he clearly recognizes, and require working with a broad spectrum of cutures and political heritages. At one point he states: "The creation of effective constitutional processes within each political community can follow the lead of liberal democratic and democratic socialist political communities over the last 200 years" (p. 192). If Riemer is going to argue that his approach to politics provides a paradigm superior to those now in existence, then it is incumbent on him to indicate specifically that it has applicability to the various political communities in the world.

Efforts such as Riemer's are exceptionally valuable in maintaining the vitality and flexibility of political discourse. In this instance, the prophetic constitutional paradigm that he describes fits reasonably well within the Western liberal democratic tradition, and, if nothing more, could serve as the basis for an alternative perspective to policies formulated in that tradition. It may also be useful outside the Western democratic tradition, but to be convincing to most readers, Riemer's claims of universality will need more support than he has provided here.

ROBERT A. HEINEMAN

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The Politics of Meaning: Power and Explanation in the Construction of Social Reality. By Peter C. Sederberg. (Tucson: University of Arizona Press, 1984. Pp. vii + 294. $22.50, cloth.)

A wide-ranging and provocative study, Sederberg's book is a loosely defined constellation of arguments derived from his contention that politics consists of intentional efforts to constitute, maintain, and control "systems of shared meaning." The root of Sederberg's contention is the observation of Morse Peckham that meaning is reflected by and embodied in response. Shared meaning, following from this, the basic stuff of human sociality, requires a corresponding system of mutually accepted and expected responses. Here politics exists as the guiding, structuring, and even coercing practices that aim toward the regulation of these responses. A "left-wing Nietzschean," the author finds such an understanding of politics to be laudable, seeing neither absolute nor consensual grounds for meaning. Hence, politics would be the sole *responsible* ground for the production and maintenance of meaning.

With this as point of departure, the remaining chapters of Sederberg's volume explore intellectual territories where his thesis that politics constitutes meaning might lend fresh and radical perspective. In this, unfortunately, it becomes plain that the study does not offer the "new paradigm of politics" promised by its jacket advertisement. Fitting comfortably within the what may now be a dominant academic approach to political theory, Sederberg's work begins, with an epistemological consideration of the character of scientific inquiry, both natural and social. Acknowledging that science too produces meaning, he must address in what way this conforms with his own thesis on politics and meaning. Much like an uncited C. S. Peirce, the argument is made that science is actually a social enterprise and that its regulation of meaning is accomplished only via a process of structure and control that is political in quality, equating methodology with ideology. Moreover, whereas the ideological character of methodology in the natural sciences is to some extent mitigated by a certain empirical aspect to observations of the natural world, the social sciences lack even this slim constraint on the ideological quality of method. Politics in science, as elsewhere, controls and maintains these systems of shared meaning.

From this sortie into the philosophy of science the volume then turns toward rendering the "politics of meaning" responsible. Despite reference to Nietzsche's claim that any meaning is preferable to none, Sederberg is surprisingly quick to judge that both unalloyed coercion and mere social consensus are inappropriate means for the regulation of meaning. Nor is the egoistic transvaluation of values by the *übermensch* judged to be appropriate. Instead, after supernumerary treks into organization theory and the politics of scarcity, the politics of meaning is revealed to be a kind of Burkean reverence for and recognition of the order of culture coupled with a Nietzschean skepticism based upon the awareness of utter human self-responsibility for the creation of social reality. For Sederberg, this is a perspective that embraces the indeterminability of all guides to human action while yet demanding that all human action be accountable to human responsibility. Conflicting interpretations of cultural values within this framework, arising from the required critical stance of skepticism, are resolved in the only possible arena of resolution—politics.

The rub, of course, is whether correct resolution of such conflict differs from incorrect resolution. *The Politics of Meaning* is rather silent on this point. In its emphasis on Nietzsche's critique of the transcendental, the work perhaps less well appreciates that Nietzsche's critique is conjoined with a positive, imminent vision of what man optimally *is.* It is an all too obvious empirical fact that politics constitutes meaning; the world speaks newspeak. But, the mere critical recognition of this and the recognition of the responsibility this insight brings in its train still courts the nihilism Nietzsche and Sederberg fear. Responsibility requires vision. Although an interesting study, *The Politics of Meaning* might well delve more deeply into Nietzsche's anthropology of the overman and the "will to power."

STEPHEN FREDERICK SCHNECK

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Politics and Remembrance: Republican Themes in Machiavelli, Burke, and Tocqueville. By Bruce James Smith. (Princeton, N.J.: Princeton University Press, 1985. Pp. xii + 287. $27.50.)

This allusive book makes a fine contribution to the revival of republican thought. Smith's book studies republicanism as a way to constitute political life by recollecting shared memories. The artful, but not arcane, textual analysis focuses upon the role of recollection, forgetting and foundings as the key to a viable citizenry in the thought of Machiavelli, Burke, and Tocqueville.

Smith adds a twist to the mandatory laments about the decline of public life that afflict this literature. He is dismayed, not that citizens do not act, but that modern democrats are unable to build durable accomplishments. For Smith, the republican tradition links durability to the affections of citizens. This "public" consists of citizens and patriots who share a civic personality. This personality is lodged in memories alive with affection for habits, goals, and institutions.

Although the essays are too loosely joined, Smith uses Machiavelli to frame his thesis. Machiavelli emphasizes the temporality of republics. Republics are founded, and our recollection of the foundations anchors civic consciousness. But each republic begins in patricidal violence. The perpetrators live in rough equality and must overcome the violence, fear, and catastrophe at the core of Machiavellian foundings. All three theorists recognize that a dialectic of ambition and envy among equals can destroy a republic. Republics unleash great energy, but they also breed hatreds, envy, and a flight to private faction.

In Smith's view, republics must reconcile authority and equality, and this is accomplished in memory when citizens internalize commitments embodied in the foundings. Alive with passionate memories, patriots revere foundations. This reverence allays envy, curtails ambition, but gives a focus for decision and action. Foundation myths equalize and humble all citizens while they generate patriotism that disciplines our love of faction.

To emphasize the past as an antidote to the turmoil of equality invites Burke's solution. Smith's Burke uses custom to obliterate active memory. Burke's conservatism is born in terror, and Burke seeks to blunt the English recovery of the memory of 1648. He safely banishes origins to the mists of time and thwarts the reappropriation of memory. Custom controls envy and ambition while it subjects memory. Although the analysis of Tocqueville seems the least focused and connected, Smith sees him as providing a modern alternative to Burke but a sobering of Machiavelli's exuberance. Smith's Tocqueville seeks to cure the disease of memory that afflicts France. The aristocracy and republicans remember their hatred and empire. Frozen in enmity and isolated by fear, they have forgotten the knowledge and devotion that made polity alive. Tocqueville reminds us that in our remembering and forgetting, none can start as clean as Machiavelli would wish. We must remember our history if we are to build well.

The beautiful and elusive language of civic republicanism can seduce and direct, but also frustrate and obscure. After his elegant textual analysis, Smith's own reappropriations all too predictably direct us to "small republics," chide democrats to take institutions and legend seriously, and warn against the hubris of dymythologizing reason that cannot build lasting foundations. But his institutions and affections have a fugitive quality. Despite his undomesticated Machiavelli, the book often romanticizes politics. Classes and violence disappear. One wonders how self-conscious individuals keep their reverence, even as they must recover and reform forgotten injustice or flawed foundations. Institutions, even foundings, deserve our affection to the extent they embody worthy goals. At some point, the insights of civic republicanism must reconcile with the contemporary defenses and extensions of equality and freedom. These concerns do not nullify, so much as arise from this work's originality.

J. PATRICK DOBEL

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Empirical Theory and Methodology

Vision and Method in Historical Sociology. Edited by Theda Skocpol. (New York: Cambridge University Press, 1984. Pp. xiii + 410. $39.50, cloth; $13.95, paper.)

This book is highly specialized, but it also illuminates a wide range of concerns and problems. It is a complex, informative survey of several important scholars who deserve treatment as a group, and it brims with valuable research leads.

The fascination of the book lies in its core of nine essays (most of them shaped by, and one coauthored, by Theda Skocpol) depicting the work and concerns of some very interesting people. Indeed, a few of these essays by younger scholars who came of age in the '60s and '70s are truly powerful evocations of the nature of social scientific insight.

Essays concern Marc Bloch, the great French historian; Karl Polanyi, political economist; S. N. Eisenstadt, theorist of social change; Reinhard Bendix, transmitter and practitioner of the values and vision of Max Weber; Perry Anderson, writer of world history; E. P. Thompson, the engaged British historian; Charles Tilly, analyst of collective action in modern social life; Immanual Wallerstein, who has daringly expanded the concept of a world capitalist system; and last but hardly least Barrington Moore, the solitary frontiersman who has done so much to show the value of comparative historical social science.

This book seems partly an intellectual manifesto: a self-confident statement by a new generation of scholars about their heroes, and thus about what this new generation does and why. But this is only partly what the book is, and not the most important part. The book's overriding concern is to argue cogently for a certain type of social science, one that seeks to understand and to explain persuasively the long-range dynamics of social and political change.

Thus a sustained focus on the logic of social scientific reasoning and its limits is common to all the essays. Consistent with the basic intention of the work, the editor and contributors take care to present the essays lucidly. Their clarity is combined with rigorous appreciation and critique. Useful bibliographies follow each essay, and at the end there is a valuable annotated bibliography of work on the methods of comparative historical social science.

Some political scientists unfamiliar with the tradition that the book follows will find reading about it rather hard going. In particular, many Americanists and behavioralists may initially feel perplexed. But in addition to strengths already mentioned, another must be noted. Because the book provides a thorough analysis of how to think historically about the big questions of political and social change, it implicitly (and often explicitly) presents and answers many interesting questions about modern politics, for example, Are there threats to American democracy contained in America's postwar economic hegemony? What are the structural historical constraints on third-world politics?

This book of edited essays offers a gallery of expertly done intellectual portraits. It also offers much more and has bearing on political science in several ways. Any political scientist who spends time with it will come away intellectually refreshed, challenged, and instructed.

RICHARD M. VALELLY

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Annotations

Marxists in Face of Fascism: Writings by Marxists on Fascism from the Inter-War Period. By David Beetham. (Totowa, N.J.: Barnes & Noble Books, 1984. Pp. 381. $29.95.)

This reader contains selections from Marxist commentaries on fascism written between the two world wars. Sections include fascism in Italy, 1921-1928; the communist international; communists in opposition, 1928-1937; social democracy and fascism; and the fascist state, the economy, and war. Many of the texts included have not been previously translated. Volume includes a lengthy introduction, chronological table, biographical notes, a list of sources, and an index.

Alexis de Tocqueville: Selected Letters on Politics and Society. Edited by Roger Boesche. Translated by James Toupin and Roger Boesche. (Berkeley: University of California Press, 1985. Pp. xiv + 417. $24.95.)

These selected letters introduce the reader to Tocqueville's youth and journey to North America, his political speculation, the writing of *Democracy in America,* and his election to the Chamber of Deputies, the politics of the July monarchy (1839-1947), the period between the February revolution and the election of Louis Napoleon, the period from Louis Napoleon to the emergence of the Second Empire, the beginnings of the Second Empire to authorship of *The Old Regime,* and Tocqueville's final years. Volume contains an introduction, biographical sketches of Tocqueville's correspondents, a chronology, a listing of text sources, and an index.

The American Military Mission in the Allied Control Commission for Bulgaria, 1944-1947: History and Transcripts. Edited by Michael M. Boll. (Boulder, Colo.: East European Monographs, and New York: Columbia University Press, 1985. Pp. ix + 332. $30.00.)

This monograph contains an extensive history of the allied control commission in Bulgaria, minutes of its extraordinary and plenary meetings, the armistice terms for Bulgaria, and an index. The documents made available here were released within the past decade.

Canadian Almanac and Directory 1985. Edited by Susan Bracken. (Detroit: Gale Research, and Toronto: Copp Clark Pitman. Pp. 1,210. $64.00.)

The 138th edition, this almanac provides comprehensive coverage of Canadian government and society. Sections include associations and societies, broadcasting and communication, church and religious organizations, commerce and finance, a cultural directory, an education directory, electoral districts, foreign and international information, geographic information, a government directory, health and hospitals, historic and general information, a legal section, postal information, tourism, and transportation. Volume contains index.

Social Science Methods in the Legal Process. By Noreen L. Channels. (Totowa, N.J.: Rowman & Allanheld, Publishers, 1985. Pp. x + 276. $39.95.)

Designed as an introduction to social science research for lawyers and related professions, this book reviews the applications of social science research in legal proceedings, compares legal and scientific modes of analysis, discusses how to ask appropriate and researchable questions, describes research designs and their validity, and provides an overview of sampling and measurement, data collection, and the use of existing data, descriptive statistics, and inferential statistics. Text concludes with a chapter on the collaboration between lawyers and social scientists. Volume includes bibliography and index.

The Effects on the Atmosphere of a Major Nuclear Exchange. Committee on the Atmospheric Effects of Nuclear Explosions et al. (Washington, D.C.: National Acaddemy Press, 1985. Pp. ix + 193. $14.50, paper.)

Prepared in response to a Department of Defense request, this report summarizes existing knowledge, draws conclusions where appropriate, and recommends additional research to reduce areas of uncertainty. The volume considers the baseline nuclear exchange itself, dust, fires, chemistry, and atmospheric effects and interactions. Volume contains index.

Nuclear Arms Control: Background and Issues. Committee on International Security and Arms Control, National Academy of Sciences. (Washington, D.C.: National Academy Press, 1985. Pp. x + 378. $16.50, paper.)

Prepared by the National Academy of Sciences' Committee on International Security and Arms Control, this volume provides an overview of the arms control process and then considers strategic offensive nuclear arms control, the nuclear freeze, intermediate-range nuclear force negotiations, strategic defense arms control, antisatellite arms control, nuclear test bans, and the nonproliferation of nuclear weapons. Volume also contains several appendixes on agreements and treaties and an index.

Edward S. Corwin and the American Constitution: A Bibliographical Analysis. By Kenneth D. Crews. (Westport, Conn.: Greenwood Press, 1985. Pp. xiv + 226. $35.00.)

This book includes a discussion of Corwin's life and times, bibliographic annotations on books written or edited by Corwin, parts of books to which Corwin contributed, his articles, letters to the editor, and his book reviews. Other bibliographic annotations cover materials written about Corwin. Volume also contains a subject index and a periodicals index.

The Cambridge History of Africa: Vol. 8, From c. 1950 to c. 1975. Edited by Michael Crowder. (New York: Cambridge University Press, 1985. Pp. xvi + 1,011. $84.50.)

The final volume in its series, this book examines the Second World War as a prelude to African decolonization, the subsequent problems of independence, pan-Africanism since 1940, African social and cultural change, economic evolution in developing Africa, Southern Africa, English-speaking West Africa, East and Central Africa, the horn of Africa, Egypt, Libya, and the Sudan, the Naghrib, French-speaking tropical Africa, Madagascar, Zaire, Rwanda, and Burundi, and Portuguese-speaking Africa. Volume contains a bibliography and index.

American Violence and Public Policy: An Update of the National Commission on the Causes and Prevention of Violence. Edited by Lynn A. Curtis. (New Haven, Conn.: Yale University Press, 1985. Pp. xi + 263. $23.00.)

Intended as an update to the Report of the National Commission on the Causes and Prevention of Violence (1969), this volume examines violence and public policy in the past 15 years. Topics include the extent and character of violent crime, changing directions in crime policy, black violence, citizen self-help and neighborhood crime prevention, presidential commissions and the Law Enforcement Assistance Administration, collective violence, and terrorism. Violence includes a statistical appendix and index.

The Documentary History of First Federal Elections: Vol. 11, 1788-1790. Edited by Gordon DenBoer. (Madison: University of Wisconsin Press, 1984. Pp. xxii + 520. $50.00.)

The second of a four-volume set, this book includes chapters on the elections in Connecticut, Delaware, Maryland, Virginia, and Georgia. Each chapter contains official documents divided into such sections as the background of the elections, the election of presidential electors and their votes, the election of senators, passage of the election laws, the election of representatives, and commentaries on the elections. Volume has an index.

Managing Interdisciplinary Research. Edited by S. R. Epton et al. (New York: John Wiley & Sons, 1983. Pp. x + 243. $29.95.)

Based on papers presented at the Second International Conference on the Management of Interdisciplinary Research, this volume includes discussions of the difference between multidisciplinary and interdisciplinary research, the organizational form and management of cross-disciplinarity, several selections on research and development, indicators of interdisciplinary research, and many more. Volume contains a list of the papers presented at the first conference, a bibliography, and a subject index.

The Far East and Australasia 1984-85. (Detroit: Gale Research, and London: Europa Publications, 1984. Pp. xiv + 945. $140.00.)

This 16th edition contains general surveys of the area (political events, population, development problems, religious, commodities, and more), information on regional organizations, and country surveys for Afghanistan, Australia, Bangladesh, Bhutan, Brunei, Burma, China, Hong Kong, India, Indonesia, Japan, Kampuchea, Korea, Laos, Macau, Malaysia, Maldives, Mongolia, Nepal, New Zealand, the various Pacific Islands, Pakistan, The Philippines, Singapore, Sri Lanka, Thailand, the USSR in Asia, and Vietnam. Volume contains several bibliographies but no index.

Models of Category Counts. By Bernard Fingleton. (New York: Cambridge University Press, 1984. Pp. viii + 187. $34.50.)

This book draws upon the literature on contingency table analysis or cross-tabulated data analysis to introduce the nonstatistician or applied statistician to these research tools. Chapters cover independence and association in the two-dimensional table, the multidimensional table, unsaturated models, sample design and inference, table design and inference, and counts from temporal observations. Volume includes references, author index, and subject index.

Canadian Legislatures: The 1984 Comparative Study. Edited by Robert J. Fleming and J. Thomas Mitchinson. (Toronto: Office of the Assembly, 1984. Pp. 76. $9.00, paper.)

This edition reports the results of a poll of Canadian citizens on attitudes toward their legislative institutons. Other chapters include a comparison between legislative salaries for state legislators in the United States and those in the Canadian provinces, results of recent elections, and such legislative information as members' indemnities, salaries, allowance, and benefits, support services for private members, administrative structures, and House statistics. Monograph also includes an index of tables.

Selected Writings and Speeches of Alexander Hamilton. Edited by Morton J. Frisch. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1985. Pp. xiv + 524. $25.95, cloth; $14.95, paper.)

This volume attempts to provide a distilled introduction to Hamilton's political thought, given that *The Papers of Alexander Hamilton* now contain 26 volumes. Selections include materials from "The Continentalist Papers," "The Federalist Papers," Hamilton's remarks and speeches at the New York ratifying convention, the "Catullus Essays," the "Tully Letters," and "The Lucius Crassus Criticism of Jefferson's Message to Congress of December 8, 1801." Volume includes an introduction and an index.

The Watergate Investigation Index: House Judiciary Committee Hearings and Report on Impeachment. Compiled by Hedda Garza. (Wilmington, Del.: Scholarly Resources, 1985. Pp. xiii + 261. $95.00.)

This guide provides ready access to select information contained in one or more of the following: Report of the Committee of the Judiciary, Minority Memorandum on Facts and Law, Testimony of Witnesses, Statement of Information and Appendices, Statement of Information, Summary of Information, Transcripts of Eight Recorded Presidential Conversations, Statement of Information Submitted on Behalf of President Nixon, and Impeachment Inquiry. Entries and sub-entries are arranged alphabetically. Both names and subjects are indexed in the guidebook.

Freedom in the World: Political Rights and Civil Liberties, 1984-1985. By Raymond D. Gastil. (Westport, Conn.: Greenwood Press, 1985. Pp. x + 438. $35.00.)

This volume marks the 12th year of the comparative survey of freedom and represents the sixth edition in the Freedom House series. Part 1 contains the comparative survey of freedom; Part

2 examines such current issues as communications and UNESCO, the 1982 and 1984 elections in El Salvador, and the presidential and legislative elections in Panama. Part 3 looks at definitions of democracy, a historical survey of freedom in America, a note on economic freedom, and an essay on the forms and formalities of liberty. Part 4 contains essays on the growth of democracy in developing countries, the democratic struggle in Central America, and the diffusion of democracy and liberal modernism. Parts 5 and 6 provide country and territory summary descriptions, respectively. Volume contains index.

Education Report Card: Schools on the Line. Edited by Hoyt Gimlin. (Washington, D.C.: Congressional Quarterly, 1985. $8.50, paper.)

This reference book includes nine reports originally prepared as Editorial Research Reports on such topics as the status of schools, colleges in the 1980s, teachers and the push for excellence, post-Sputnik education, illiteracy in America, schoolbook controversies, school prayer, pressures on youth, and the changing environment in college sports. Volume includes selected bibliography and an index.

Foreign Relations of the United States, 1952-1954: Volume 14, Part 1 and Part 2. Edited by John P. Glennon. (Washington, D.C.: U.S. Government Printing Office, 1985. Pp. xx iii + 1, 845. $28.00.)

This massive two-part compilation of documents covers China (Part 1) and Japan (Part 2). The China volume reviews U.S. policy toward the People's Republic of China and the Republic of China, the change of directive to the Seventh Fleet, relations between the two Chinas and the U.S. and the People's Republic, offshore island problems, negotiation of a mutual security treaty with the Republic of China, and more. The Japanese volume covers U.S. relations and policies toward Japan, various treaties and agreements with Japan, issues surrounding Japanese rearmament, its peace treaty with China, economic and military assistance to Japan, trade and investment in Japan, and Japanese relations with Korea.

Essays on the Modern Law of War. By L. C. Green. (Dobbs Ferry, N.Y.: Transnational Publishers, 1985. Pp. xxii + 281. $37.50.)

This treatise reviews the emerging laws of modern war under such topics as the new law of armed conflict, ignorance of the law is no excuse, superior orders and the man in the field, legal advisors and the armed forces, human rights and armed conflict, war law and the medical profession, aerial considerations, lawful and unlawful weapons and activities, the status of mercenaries in international law, war crimes, extradition, and command responsibility, armed conflict and the enforcement of international criminal law, Canadian law and the punishment of war crimes, and Canada's role in the development of the law of armed conflict. Volume contains a table of cases and an index.

Troubled Lives: John and Sarah Austin. By Lotte Hamburger and Joseph Hamburger. (Toronto: University of Toronto Press, 1985. Pp. xv + 261. $27.50.)

This account presents a dual portrait of John and Sarah Austin, their lives and times, his opinions and beliefs on such matters as politics, law, democracy, and constitutionalism, her opinions and beliefs on such matters as politics, democracy, equality, marriage, and the position of women, their close friends (e.g., John Stuart Mill), and much more. Volume includes an index.

The Marcus Garvey and Universal Negro Improvement Association Papers: Vol. 3, **September 1920-August 1921.** Edited by Robert A. Hill. (Berkeley: University of California Press, 1984. Pp. lviii + 811. $39.50.)

The third of a 10-volume survey, this volume includes documents arranged chronologically from September, 1920 to August, 1921. These materials cover the period between the first and second international conventions of the Universal Negro Improvement Association. Selections include letters, reports by government agents, newspaper articles, diplomatic communications, and more. Volume also contains documents relating to Garvey's organization and an index.

Peacekeeper's Handbook. International Peace Academy. (New York: Pergamon Press, 1984. Pp. x + 439. $50.00.)

This handbook provides an overview of U.N. entities concerned with the maintenance of international peace and security, the principles and procedures for mounting U.N. peacekeeping operations, standard operating procedures, operational handling and techniques, logistics, communications, military economic operations and humanitarian relief, preparation and preparedness, civilian police in peacekeeping operations, information and public relations, the law and international peacekeeping, the attitude of the peacekeeper, and planning and procedures for medical services. Volume includes index.

China and Thailand, 1949-1983. Edited by R. K. Jain. (Atlantic Highlands, N.J.: Humanities Press, 1984. Pp. xxvi + 413. $30.00.)

Utilizing some 425 documents from official Chinese and Thai sources, this volume examines Chinese policy toward Thailand during the period from 1949 to 1983. The editor's introduction provides a general overview of Chinese relations with Thailand. Volume also includes a list of document sources; appendixes on exchange visits, agreements, and trade, a select bibliography, and an index.

Political Risk Assessment: An Annotated Bibliography. Compiled by David A. Jodice. (Westport, Conn.: Greenwood Press, 1985. Pp. xii + 279. $35.00.)

Focusing largely on publications between 1970 and 1983, this guide provides annotated entries on political risk overseas and how multinational companies and financial institutions have responded. Chapters include an overview of political risk assessment, the multinational corporation in world politics, the sources of political risk, analyzing and managing political risk, and sources of data and research materials. Volume contains an index.

The Year Book of World Affairs 1984. Edited by George W. Keeton and Georg Schwarzenberger. (Boulder, Colo.: Westview Press, 1984. Pp. vii + 377. $42.00.)

The 38th in its series, this volume is the last scheduled for publication. Articles include an historical review of the London Institute of World Affairs, a look at power politics and international order, several articles on the Falklands conflict, the American diplomatic initiative in the Middle East, pluralism and foreign policy, the new international economic order, issues of nuclear war and control, and Beirut, 1982. Volume also contains a cumulative index for volumes 26-38 in this series.

Documents on the Israeli-Palestinian Conflict, 1967-1983. Edited by Yehuda Lukacs. (New York: Cambridge University Press, 1984. Pp. xi + 247. $29.95.)

This compilation of key documents includes 11 documents reflecting U.N., European, and Soviet positions, 10 on the U.S. position, six involving Sadat's visit and autonomy negotiations, 13 Israeli documents, 14 Israeli party and extraparliamentary platforms, 19 Palestinian documents, and 16 on the Arab position. Volume contains no index.

New Communication Technologies in Politics. Edited by Robert G. Meadow. (Washington, D.C.: Washington Program of the Annenberg School of Communications, 1985. Pp. 145. $15.00, paper.)

Based on papers presented at a conference on Communication Technologies and Politics, this monograph contains essays on political campaigns, new technologies and political competition, innovations in campaign research, new techniques in computerized voter contact, direct mail and fund raising with new technologies, from telephone to telelobby, the architecture of campaign software, an annotated bibliography of direct mail, and an annotated bibliography of new technology and politics. Volume has no index.

New Local Centers in Centralized States. Edited by Peter H. Merkl. (Lanham, Md.: University Press of America, 1985. Pp. 356. $27.50, cloth; $14.75, paper.)

This collection is designed to provide the reader with examples of strengthened local centers not commonly covered in the English-language literature on trends in local government. Four chapters examine local changes in small towns in West Germany, and five other chapters provide case studies from France, The Netherlands, and Norway, Italy, and India. Volume contains brief introduction and conclusion but no index.

The Survey Method in the Social and Political Sciences: Achievements, Failures, Prospects. By William L. Miller. (New York: St. Martin's Press, 1983. Pp. ix + 259. $27.50.)

Designed as a text for the student and general reader, this book looks at the survey method in general, examines arguments for and against surveys, analyzes presidential election surveys since 1940, parliamentary election surveys since 1950, and cross-national surveys or political participation, and suggests new applications, alternatives, and technologies. Text contains references and an index.

Essays on French History and Historians. By John Stuart Mill. Edited by John M. Robson. (Toronto: University of Toronto Press, 1985. Pp. cxix + 517. $65.00.)

The materials collected here were written between 1826 and 1849 and illustrate Mill's interest in French history, politics, and society. Many of the documents are extended commentaries by Mill on British and French historians of France. Volume contains a general introduction and a textual introduction, various appendixes that include some of the materials upon which Mill commented, and an index of persons and works cited and a general index.

High Technology Policies: A Five-Nation Comparison. By Richard R. Nelson. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1984. Pp. xiii + 94. $13.95, cloth; $4.95, paper.)

This monograph analyzes the strengths and weaknesses of various industrial policies toward semiconductor and computer industries, civil aircraft industries, and nuclear power industries in Britain, France, Japan, West Germany, and the United States. Volume includes a bibliography but no index.

Vital Statistics on Congress, 1984-1985 Edition. Edited by Norman J. Ornstein et al. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1984. Pp. xxiii + 261. $19.95, cloth; $9.95, paper.)

Now in its third edition, this volume provides detailed aggregate information on the members of Congress, congressional elections, campaign finance, committees, congressional staff and operating expenses, workload, budgeting, voting alignments, and appendixes on individual members of the 97th and 98th Congresses. Each chapter includes a brief narrative in addition to numerous tables and charts. Volume has no index.

Democracy Must Work: A Trilateral Agenda for the Decade. By David Owen et al. (New York: New York University Press, 1984. Pp. 88. $12.50.)

A report to the Trilateral Commission, this volume examines dilemmas of the decade, the social response to employment and equity, the economic response to interdependence and growth, the political response to cooperation or fragmentation, and tasks and tradeoffs facing Trilateral partners. Volume contains no index.

Governments in Democratic Capitalist States 1950-1983: A Data Handbook. By Heikki Paloheimo. (Turku, Finland: University of Turku, 1984. Pp. vii + 235. Price unavailable; paper.)

This text provides separate country tables for each of 24 nations on political parties (names, abbreviations, years of parliamentary status, and years in the government) and governments (date of nomination, period in office, name of prime minister, party of prime minister, party membership of ministers, and parliamentary support). Each country chapter includes a brief discussion of its constitutional structure, parties, and governments. Some cross-national comparisons are contained in appendixes. Volume includes references but no index.

The Transformation of Democracy. By Vilfredo Pareto. Edited by Charles H. Powers. Translated by Renata Girola. (New Brunswick, N.J.: Transaction Books, 1984. Pp. 128. $12.95, paper.)

Originally published in 1920, these serialized chapters include Pareto's "Generalizations," "The Crumbling of Central Authority," "The Plutocratic Cycle," and "Sentiments." This English translation includes an introduction by the editor, a brief epilogue, and an index.

The Middle East Annual: Issues and Events: Vol. 3, **1983.** Edited by David H. Partington. (Boston: G. K. Hall, 1984. Pp. xii + 269. $474.00.)

The third in its series, this volume aims to provide an event chronicle plus analysis for the modern Middle East. Chapters include the events of 1983, Islamic fundamentalism, the PLO since Beirut, the Soviet Union, Syria, and the crisis in Lebanon, Chad, the Middle East in the U.S.

media, and a bibliography of publications in Middle Eastern studies for 1983. Volume contains an index.

State Government Export Promotion: An Exporter's Guide. By Alan R. Posner. (Westport, Conn.: Quorum Books, 1984. Pp. x + 192. $29.95.)

This volume examines the economic development function of state governments in promoting export development. Chapters examine state-level export promotion, why it is important, the states and transnational economic relations, and state services to small and medium firms. Appendixes list state export promotion agencies and promotion services. Volume contains bibliography and index.

Soviet-U.S. Relations: The Selected Writings and Speeches of Konstantin U. Chernenko. Compiled and edited by Victor Pribytkov. (New York: Praeger Publishers. Pp. viii + 203. $21.95.)

This compilation covers a wide array of topics, among them the Conference in Helsinki and international security, the Leninist strategy of peace in action, addresses to various Communist party congresses, Soviet-Cuban friendship, human rights, the ideology of revolutionary creative effort and peace, and many more. Volume contains no index.

Records Management Handbook for United States Senators and Their Repositories. (Washington, D.C.: United States Senate Historical Office, 1985. Pp. vii + 130. Free; paper.)

This handbook proposes procedures for the management of senatorial papers with an eye to subsequent archive use of these private papers. Chapters include an overview of file management policy, the design and implementation of file maintenance and disposition, the management and disposition of automated records, techniques of file management micrographics, subject files and indexes, courtesy storage at the Washington National Records Center, and donating paper collections. Volume includes various appendixes and a list of suggested readings but no index.

Fascism and Pre-Fascism in Europe, 1890-1945: A Bibliography of the Extreme Right. Edited by Philip Rees. (Totowa, N.J.: Barnes & Noble Books, 1984. Pp. xxii + 329. $29.95.)

This guide with brief annotations contains chapters on fascism in general and country chapters on Norway, Denmark, Sweden, Finland, Iceland, Ireland, France, Belgium, The Netherlands, Luxemburg, Portugal, Spain, Switzerland, Austria, Greece, Eastern Europe generally, Albania, Bulgaria, Yugoslavia, Romania, Hungary, Poland, Czechoslovakia, Russia and the Ukraine, the Baltic states generally, Estonia, Latvia, Lithuania, Italy, and Germany. Although numerous, entries focus on often-cited and significant materials on the extreme Right's ideology and practice. Most entries involve books, dissertations, or periodical articles; selection favors post-1945 analyses of the 1890 to 1945 period. Volume contains a personal name index.

Guanzi: Political, Economic, and Philosophical Essays from Early China: Vol. 1, Chapters I, 1-XI, and XX, 64-XXI, 65-66. Edited and translated by W. Allyn Rickett. (Princeton, N.J.: Princeton University Press, 1985. Pp. x + 484. $50.00.)

The first of a proposed three-volume set on the Guanzi and the first of two devoted to its translation, these tracts from ancient Chinese political and philosophical works were compiled around 26 B.C. by Han dynasty scholar Liu Xiang. This volume includes an introduction by the editor, extensive notes, a bibliography, and an index.

American Third Parties since the Civil War: An Annotated Bibliography. By D. Stephen Rockwood et al. (New York: Garland Publishing, 1985. Pp. 194. $25.00.)

This annotated bibliography includes chapters on the theory and practice of third parties, the Populist party, parties of the Left, Dixiecrats and American Independents, the Progressive party, and minor third parties. The authors emphasize book-length materials, although major periodical selections also are included. Additional periodical entries are provided for third-party movements during the past two decades. Coverage of socialist parties begins in 1952, and readers are referred to Donald D. Egbert and Stow Parson, eds., *Socialism and American Life* (Princeton University Press) for coverage before that point. Volume includes both an author and title index.

The Arms Race at a Time of Decision: Annals of Pugwash 1983. Edited by Joseph Rotblat and Alessandro Pascolini. (New York: St. Martin's Press, 1984. Pp. xxiv + 291. $30.00.)

The first in a proposed series, this volume includes papers from the 1983 meeting of the Pugwash Conference on Science and World Affairs. Contributors include scientists from East and West and from the natural and social sciences. Volume sections cover space militarization, the strategic nuclear arms race, security in Europe, problems of regional security, and security-related third-world problems. Volume contains glossary and index.

Population: The UNFPA Experience. By Nafis Sadik. (New York: New York University Press, 1984. Pp. xvii + 206. $25.00.)

This volume records UNFPA's experience with international population assistance. In addition to a general introduction to UNFPA's role, the text includes regional outlooks for sub-Saharan Africa, Asia and the Pacific, Latin America and the Caribbean, Middle East and the Mediterranean, and Europe. Program chapters cover family planning, information, population education and communication, population data collection, demographic research, training, and population policy, and women, population, and development. Volume contains index.

Governments and Multinationals: Policies in the Developed Countries. By A. E. Safarian. (Washington, D.C.: British-North American Committee, 1983. Pp. x + 117. $8.00, paper.)

This monograph considers developed country (Australia, Belgium, Canada, France, the Federal Republic of Germany, Japan, Ireland, Italy, Norway, Sweden, Switzerland, the United Kingdom, and the United States) policies toward multinationals with special reference to sectors partly or completely closed to such firms, authorization and review mechanisms, treatment different from that accorded domestic firms after establishment, and investment incentives. Volume contains informational appendixes and a bibliography but no index.

Reflections on Population. By Rafael M. Salas. (New York: Pergamon Press, 1984. Pp. xv + 240. $30.00.)

Written by the executive director of the United Nations Fund for Population Activities, this book contains excerpts from Salas's policy statements on behalf of UNFPA organized analytically into chapters on fertility, the status of women and the family, morbidity and mortality, population distribution, internal and international migration, population growth and structure, promotion of knowledge and implementation of policies and programs, and international cooperation and the role of UNFPA. Appendixes include Salas's reports on the state of world population for years 1980 through 1984, other selected statements, and a list of major statements since 1979. Volume includes index.

Causal Modelling in Nonexperimental Research: An Introduction to the LISREL Approach. By Willem E. Saris and L. Henk Stronkhorst. (Amsterdam: Sociometric Research Foundation, 1984. Pp. xiii + 335. Dfl. 39.90.)

This text reviews the basic elements of causal theories, discusses their formulation, illustrates the formulation of linear structural equations and structural equation models, examines the relationships between covariances and structural parameters, introduces identification, sampling, estimation, and testing, reviews the correction of rejected theories, and shows how to interpret results and determine whether the causal mechanism has been revealed. Volume includes statistical appendixes, bibliography, and an index.

Teaching about American Federal Democracy. Edited by Stephen L. Schechter. (Philadelphia: Publius Books, 1984. Pp. 103. Free; paper.)

Designed as a supplementary modular aid for courses on American government and history, this monograph contains three essays on the principles of federalism and several additional essays applying these themes to such subjects as the party system, the Constitution, the local community, citizen education, and the role of the states. Volume contains no index.

In Nobody's Backyard: Maurice Bishop's Speeches, 1979-1983. Edited by Chris Searle. (London: Zed Books, 1984. Pp. xli + 260. $29.50, cloth; $10.75, paper.)

These selected speeches of the New Jewel Movement in Grenada are drawn from the period that Bishop served as prime minister and focus on the two years immediately preceeding his death. Topics include destabilization, health, agricultural development and regional cooperation, and cultural sovereignty of the Caribbean people. Volume contains an introduction and two brief appendixes but no index.

By Word of Mouth: 'Elite' Oral History. By Anthony Seldon and Joanna Pappworth. (New York: Methuen, 1983. Pp. xi + 257. $25.00, cloth; $12.95, paper.)

Designed as an introduction to the methodology of oral history, this book examines the strengths and weaknesses of oral history, discusses the methodology, evaluation, and use of oral evidence, and reviews several case studies in the form of books and oral archives. Book contains a brief appendix on legal considerations and another on the media and oral history but no index.

U.S. Foreign Policy and the Third World: Agenda 1985-86. Edited by John W. Sewell et al. (New Brunswick, N.J.: Transaction Books, 1985. Pp. x + 160. $19.95, cloth; $12.95, paper.)

The tenth in its series, this overview includes chapters on U.S. reassertionism, U.S. macroeconomic policy and the developing countries, international finance and investment, trade with developing countries, U.S. foreign aid in the 1980s, and third-world radical regimes. Volume contains statistical annexes and a brief glossary but no index.

Thorstein Veblen: A Reference Guide. By Jerry L. Simich and Rich Tilman. (Boston: G. K. Hall, 1985. Pp. xix + 217. $39.50.)

This guide annotates many of the writings about Thorsten Veblen between 1891 and 1982. Text includes a brief introduction to Veblen and his writings and an index. Entries are arranged chronologically and alphabetically by author within each year. The relatively comprehensive materials cited here include books, chapters or portions of books, dissertations, articles or significant portions thereof; the book reviews where the reviewer was making an independent assessment of Veblen.

Sino-Soviet Conflict: A Historical Bibliography. (Santa Barbara, Calif.: ABC-Clio Information Services, 1984. Pp. 204. $28.50.)

This annotated bibliography provides 842 selected history and social journal entries written during the last two decades that cover the Sino-Soviet split between 1914 and the present. Entries are listed alphabetically by author. Volume includes a detailed subject index and an author index.

State of the World 1985: A Worldwatch Institute Report on Progress toward a Sustainable Society. Edited by Linda Starke. (New York: W. W. Norton, 1985. Pp. xvii + 301. $8.95, paper.)

The second in its series, this report examines such topics as economy/ecosystem interaction, reducing hunger, managing freshwater supplies, maintaining world fisheries, protecting forests from air pollution and acid rain, conserving biological diversity, increasing energy efficiency, harnessing renewable energy, and stopping population growth. Volume contains numerous tables and figures and an index.

Electronic Media: A Guide to Trends in Broadcasting and Newer Technologies 1920-1983. By Christopher H. Sterling. (New York: Praeger Publishers, 1984. Pp. xxix + 335. $34.95, cloth; $17.95, paper.)

This reference book covers quantitative trends in the electronic media industry under such rubrics as the growth of electronic media, ownership of electronic media, electronic media and economics, electronic media employment and training, trends in electronic media content, the audience for electronic media, international aspects of electronic media, and regulation of electronic media. Volume contains narratives, tables, and a section on sources and references but no index.

Concepts of Federalism. By William H. Stewart. (Lanham, Md.: University Press of America, 1984. Pp. 239. $23.75.)

This book provides an inventory of figurative and nonfigurative conceptualizations of federalism. Volume includes an annotated dictionary of phrases involving the term *federalism,* such as "abortive federalism," "abstention federalism," and so on. Sources of the various terminological uses are found in a bibliography. Volume contains name and geographic index.

Taxation, Inflation, and Interest Rates. Edited by

Vito Tanzi. (Washington, D.C.: International Monetary Fund, 1984. Pp. xxiv + 247. $20.00, cloth; $15.00, paper.)

Signed background papers and a composite overview chapter examine the behavior of interest rates in selected industrial countries, their impact on capital movement, and the consequent impact on exchange rates and trade flows. Two chapters survey the literature on the impact of taxation and inflation, three examine the situation in the United States, and three others focus on such topics as interest rates in other industrial nations and international capital flows. Volume contains author and subject index.

Regulatory Regimes in Conflict: Problems of Regulation in a Continental Perspective. Edited by Fred Thompson. (Lanham, Md.: University Press of America, 1984. Pp. 170. $21.75, cloth; $11.25, paper.)

The result of one meeting of an ongoing workshop between the Canadian studies program at Columbia University and the Canadian Institute of International Affairs in Toronto, this volume examines such topics as regulating toxic substances in Canada, Canadian airline deregulation, the role of interest groups in regulatory politics, and regulatory reform in Canada. Volume includes references but no index.

Review of U.S. Military Research and Development 1984. Edited by Kosta Tsipis and Penny Janeway. (McLean, Va.: Pergamon-Brassey's International Defense Publishers, 1984. Pp. 227. $25.00.)

This review aims to provide technically correct information about ongoing military research and to herald unnecessary, destabilizing, or technologically unsound weapons systems. Chapters address the decision process for U.S. military research and development, quality in U.S. military R&D, the high speed integrated circuit program, technologies for conventional antisatellite weapons, the technology of ballistic missile reentry vehicles, defense against ballistic missiles, and understanding antisubmarine warfare technologies. Volume contains no index.

Alerting America: The Papers of the Committee on the Present Danger. Edited by Charles Tyroler, II. (McLean, Va.: Pergamon-Brassey's International Defense Publishers, 1984. Pp. xxii + 373. $28.00, cloth; $16.00, paper.)

This volume provides a documentary introduction to the Committee on the Present Danger, its positions, and its impact. These committee documents cover the years between 1976 and 1984. The volume includes appendixes on U.S. media reaction to the committee and foreign press reaction. Volume contains an index.

The Emergence of Democracy: A Comparative Study of 119 States, 1850-1979. By Tatu Vanhanen. (Helsinki: The Finnish Society of Sciences and Letters, 1984. Pp. 168. Price unavailable; paper.)

This monograph aims to explain the variety of democratic systems that have emerged between 1850 and 1979. Text has three parts, one on theoretical foundations and research design, a second on empirical analyses, and a third on the prospects for future growth of democracy. The author theorizes that democracy is most likely to emerge where power resources are widely distributed and least likely to emerge where power is concentrated. Volume contains a data appendix and bibliography but no index.

Law and Legal Information Directory. Edited by Paul Wasserman and Steven Wasserman. (Detroit: Gale Research, 1984. Pp. xvi + 902. $225.00.)

This third edition contains more than 7,700 entries under such topics as national and international organizations, bar associations, bar examination and admission to legal practice requirements, the federal court system, the highest state courts, federal regulatory agencies, law schools, continuing legal education, paralegal education, scholarships and grants, awards and prizes, special libraries, information systems and services, research centers, legal periodical publications, book and media publishers, and speaker bureaus.

Four Texts on Socrates: Plato's *Euthyphro, Apology,* and *Crito* and Aristophanes' *Clouds.* Translated by Thomas G. West and Grace Starry West. (Ithaca, N.Y.: Cornell University Press, 1984. Pp. 186. $27.50, cloth; $4.95, paper.)

This modern English translation provides a collection of otherwise separately available earlier translations of works by Plato and Aristophanes. Each translation includes numerous interpretive notes. Volume includes an introduction by the translator and a selected bibliography but no index.

Whitaker's Almanack 1985: The Year Book. (Detroit: Gale Research, and London: J. Whitaker & Sons, 1985. Pp. xii + 1,220. $14.00.)

The 117th in its series, this volume provides considerable information on British government and society. The complete edition also includes several additional pages that cover such topics as the British Commonwealth of Nations, dependent territories, basic information on foreign countries, the United Nations, sports and the arts, the media, British architecture, science, education, trade unions, and more. Volume contains index.

Latin American Population and Urbanization Analysis: Maps and Statistics, 1950-1982. By Richard W. Wilkie. (Los Angeles: UCLA Latin American Center Publications, University of California, 1984. Pp. xxi + 431. $65.00.)

This volume serves as a sourcebook to urbanization in Latin America through maps, pictures, and statistical tabulations. Overview chapters consider Latin, Middle and South America, respectively. Country chapters cover Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, the Dominican Republic, Ecuador, El Salvador, Guatemala, Haiti, Honduras, Mexico, Nicaragua, Panama, Peru, Uruguay, and Venezuela. Volume contains lists of photos, maps, and tables but no index.

Presidential Elections in Maryland. By John T. Willis. (Mt. Airy, Md.: Lomond Publications, 1984. Pp. xviii + 290. $18.50.)

This book covers every presidential election in Maryland between 1789 and 1980. Chapters contain narratives and aggregate state-level data. Appendixes provide disaggregated data by subdivision for elections, voter registration, party support, and more. Volume contains a selected bibliography, name index, and subject index.

World Development Report 1984. (New York: Oxford University Press, 1984. Pp. xi + 286. $20.00, cloth; $8.00, paper.)

The seventh in this annual series, this edition contains a special essay on population change and development. Volume also examines recovery or relapse in the world economy and provides an annex on world development indicators. Volume contains lists of tables, figures, boxes, and maps but no index.

World Economic Outlook: April 1985. (Washington, D.C.: International Monetary Fund, 1985. Pp. ix + 281. $12.00, paper.)

Published annually since 1980, this survey looks at the ongoing recovery in industrial countries, debt, adjustment, and growth in developing countries, and various medium-term scenarios for the world economy to 1990. Volume includes a large statistical appendix but no index.

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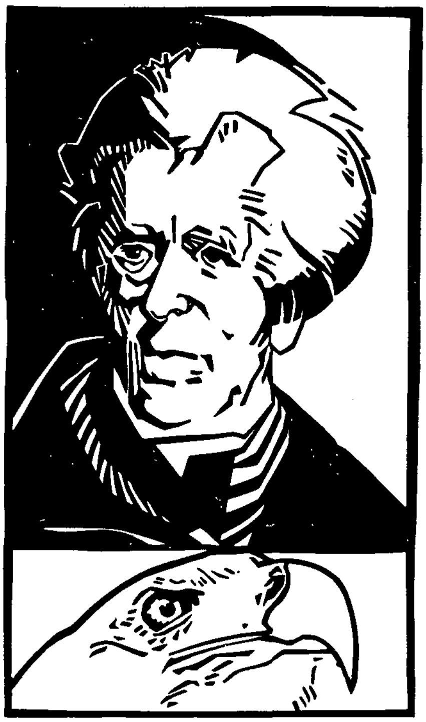

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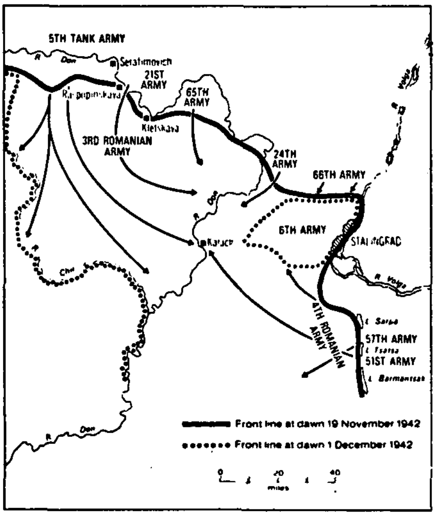
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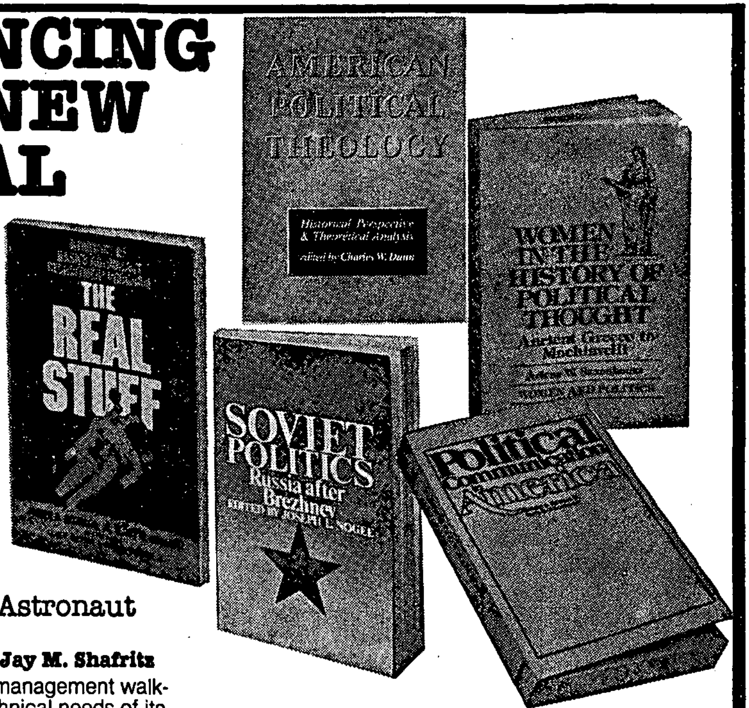
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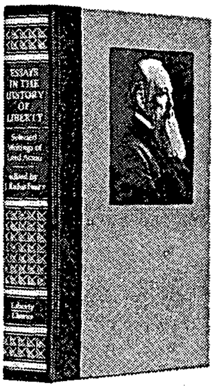

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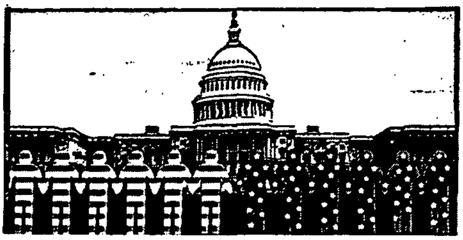

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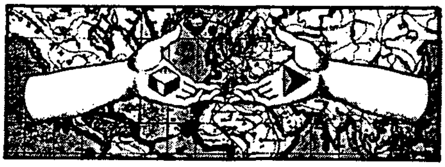

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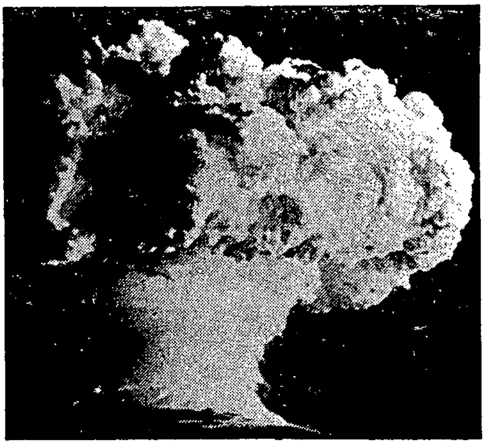

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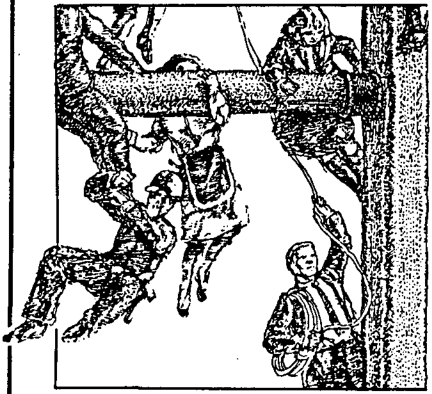

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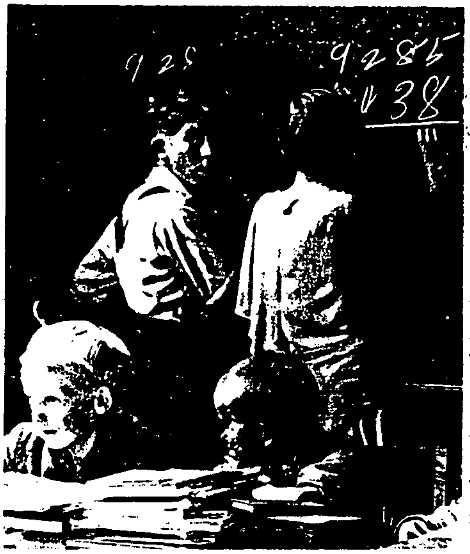

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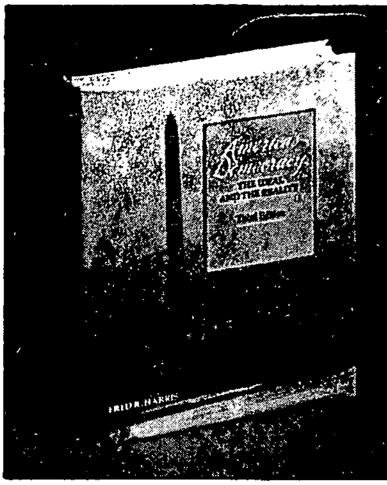

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Q: If these opportunities include interest groups, the students just cite this as another example of the fact that money talks.

A: Money talks, yes. But so do numbers, letters, rallies, editorials, petitions, media investigations, and just plain political conversation. One of the main problems in teaching American politics is that there is (usually) no "they" that calls the shots and sets the policies. The system is so messy it is hard to describe. It's even too messy to be called "pluralist" in any simple sense of that term.

Q: Do we know enough about these matters for a textbook to come to some conclusion about how much power interest groups have?

A: Not as much as one would like to know. The political system is awash in money, but it's not clear that the money buys very much. I argue that *ideas* make as much difference as material interests in explaining what happens in the political system. The text tries to summarize what we know about the impact of money and special-interest pressures. That summary requires the reader to learn to make distinctions, to classify policies into types.

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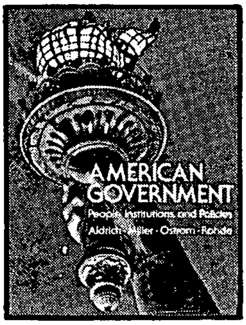
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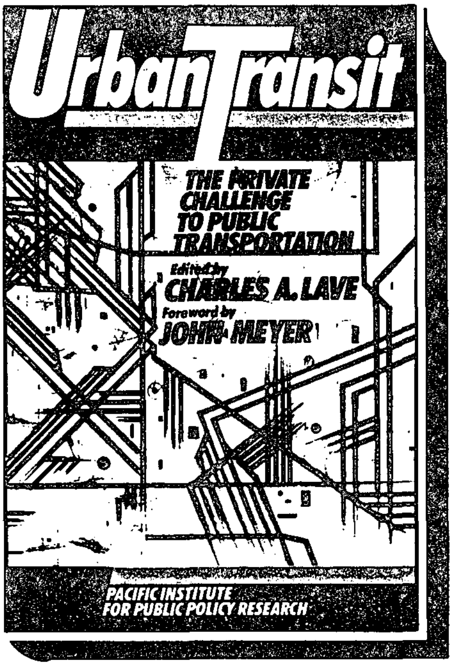
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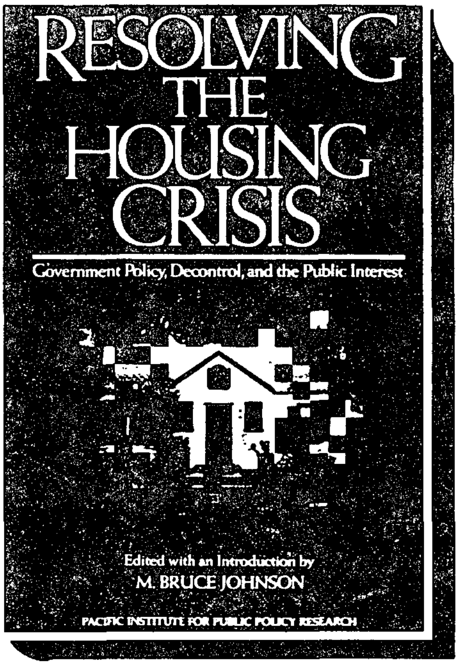
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